

# MOTHER TONGUE

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## ON THE FRONTIERS OF THE EMERGING SYNTHESIS: A TALK WITH OFER BAR-YOSEF

HAL FLEMING

*Gloucester, Massachusetts, USA*

It is no secret that our collective efforts do involve clusters of scholars working closely together, rather than some massive assembling of rugged individualists working separately. Seeing the ways in which people cooperate on some of the frontiers can set a linguist to salivating in envy. Several sectors of cooperation are those of paleoanthropologists with archeologists, biogeneticists with archeologists, and biogeneticists with a very few historical linguists. The hardware-oriented language origins scholars tend to associate with paleoanthropologists first and rather less with biogeneticists and archeologists.

Historical linguists as a whole are mostly passive recipients of ideas coming from the primary interactors. The linguists' data and conclusions are usually light years behind the others and hence usually irrelevant to whatever is being discussed. The valiant efforts of some linguists to keep up are in fact noticed and appreciated by the other scholars — well, usually. I have been turned away with considerable coolness and minimum politeness by several biogeneticists who obviously wondered how in heavens name a historical linguist could help them. The usual fear of linguistic jargon and our preoccupation with mysterious minutiae which they cannot relate to; these seem to block our cooperation.

Yet there are noteworthy and fruitful cooperations. In Russia, we have Shnirelman and Militarëv generating useful hypotheses on Dravidian and Afroasiatic. In California, there is the well-known quintet of Greenberg, Zegura, Cavalli-Sforza, Ruhlen, and Turner (Christy is from Arizona). That is actually one trio and two pairs. At Yale, Ben Rouse has the longest record of cooperation with linguists and ethnologists of anyone. Colin Renfrew and Roger Blench in England, along with their great effort in getting linguists and archeologists (mostly) to join together in the forthcoming World Archeological Congress in New Delhi, will greatly advance the cause of cooperation.

Still the most productive cooperation (yes, some competition too!) comes from the archeologists, fossil forensic folk, and the biogeneticists focused on the early man problem in the western Old World, especially Europe, Africa, and western Asia. So "hot" is the frontier there that ordinary means of communication are tending to fall behind. Many books are coming out and becoming obsolete in a few years. Many conferences are being held. And there is even a special conference chaired by John Yellen of the National Science Foundation (USA) from time to time to stimulate even more work and to get consensa more quickly.

Arguably, the most active person trying to pull together the data and conclusions of archeology and paleoanthropology is Ofer Bar-Yosef of Peabody Museum. Of course, the names of Stringer and Wolpoff leap to mind here.

And many others close behind. So let us rephrase it. One of the most knowledgeable of our scouts on the frontiers, the Western Front so to speak, is Bar-Yosef. But, unlike the others, he lives close by and I can visit him easily.

### A Short Proviso

So I did. We talked. With many interruptions and walks down side streets and other irrelevancies. So nothing can be quoted directly. Nevertheless, Ofer's scouting report was rich and very stimulating. His conclusions are still molten and cannot be put in rigid molds just yet. There is only limited bibliography; some books he suggested were particularly important. In an unpublished manuscript, there is a *rich* bibliography of things *he* has been reading (and writing too) which we will reproduce another time — with his permission.

May I suggest that the reader think of Ofer's remarks as hunches and opinions about a number of things, instead of formal hypotheses? Educated guesses by an erudite appraiser. Speculations that only a fool would ignore. Or words to that effect.

### Where is the bouncing ball going now? The Western Front:

Okay, we're focused on the early modern people of Israel of around 100,000 years ago. Where did they come from? Who are they like or who are they most like? Whose culture (archeologically) most resembles theirs? Or is there any culture that resembles theirs somewhat? They of Qafzeh and Skhul we will call "moderns".

Well, there is no doubt that the "moderns" had a Mousterian culture generally. They are associated with "Tabun C" or Middle Mousterian. However, Mousterian has been a very broadly defined or loosely defined industry for a long time now. To say that two peoples share a Mousterian industry is to make a typological statement more than anything else. Of three Mousterian industries, two may be much more like each other than the third. They might be descended from each other or they might be called closely related. If there is a general typological similarity among Mousterian cultures, and they are generally correlated with Neanderthal or Neanderthaloid physical remains, it does not follow that all Mousterian industries were made by Neanderthals only. The Qafzeh and Skhul peoples are exceptions, at least.<sup>1</sup>

But archeology has always had a label problem, especially as between lithic ages of Europe and Africa. Ofer thinks that some Middle Stone Age (MSA) cultures from South Africa were more like, or at least just as much like, the Qafzeh levels as any northern Mousterian. For example, "the Cave of Hearths is very Middle Eastern looking." Not wanting to be overwhelmed by Eurocentric labels which were inappropriate to Africa, African archeology long ago got its own set of labels. A problem has developed out of this admirable independence, however; similarities between Africa and Europe may be overlooked (mentally) even though they are clear enough (visually). Or an archeologist from Africa, for example, can

look at a tray of artifacts from some European site and note their similarity but declare them unrelated because they have different labels. "My word, your Mousterian certainly looks a lot like my MSA. But of course they could not be related, since African MSA is not correlated with the Mousterian."

Ofer's hunch then is that the wide range of African cultures from (say) Ethiopia down through eastern Africa to South Africa was the source of the so-called Mousterian of Qafzeh. That same African distribution is the one associated with the fossil finds of modern humans or nearly modern humans from (say) 150,000 years ago to perhaps 80,000 years ago. Ergo, anatomically modern people probably evolved primarily in eastern Africa and migrated or expanded northward to the Levant. Not necessarily from Ethiopia or the Sudan and down the Nile to Egypt. Western Arabia has always been an alternative route.

That eastern African distribution, associated quite frequently with the African Rift, is also associated with the general range of the Australopithecines, *Homo habilis* (*sensu latu*) and the early *Homo erectus*. Elizabeth Vrba of Yale has sent us a map which generally reflects this and hypothesized movements of *Homo erectus* to the rest of the world — this will appear in the next issue of *Mother Tongue*.

Well, what about the alternative hypotheses? First, that the "moderns" evolved out of the general Mousterian matrix culturally and out of Neanderthals physically. Second, that the Neanderthals mixed so much with the early "moderns" in the Levant over many thousands of years that the two human types became indistinguishable and that modern *Homo sapiens* in the form of Cro-Magnon was descended from the Levantine mixing. And all modern people are descended from those Cro-Magnon.

Ofer did not think that the Qafzeh culture developed out of the general Mousterian matrix, if that means out of Near Eastern or European Mousterian. That's an archeological opinion. On the question of "moderns" evolving, Ofer shrugged and said he could not be authoritative because he was not a paleoanthropologist — he felt unqualified to judge fine points of skulls and other bones when the proper experts were already fighting with each other over those bones. But he did observe that biogeneticists had been right before in their disputes with paleoanthropologists on the question of hominid splits with chimpanzees and gorillas.

But the second alternative was resisted much more stoutly. He did not think that the so-called mixture in the Levant of Neanderthals and "moderns" really happened the way many scholars say it did. What Ofer proposed was much more interesting. It went something like this:

"Moderns" occupied the Levant before Neanderthals did, say around 100 kya. Who did they replace there? Maybe some late *Homo erectus* or possibly "Archaic *Homo sapiens*" but it is not known. Levant means its usual Israel, the Jordan river valley, Lebanon, and coastal Syria. Or El Shark in Arabic.

The basic dynamics of glacial periods in the late Pleistocene was such that European and/or western Asian populations moved south during glacial periods. The notions of

robust Neanderthals and Cro-Magnons living under severe Arctic conditions is too romantic. What they did was move to warmer climes, like the Levant or down to the Black Sea lowlands (from the Caucasus or Anatolia), when it got too cold up north.

Varieties of Neanderthal, not necessarily exactly like the classical Neanderthals of Europe, moved into the Levant around 80 or 75 kya. There they either mixed with or conquered or pushed out the "moderns" who lived there. This period is the source of the confusion about the mixing of Neanderthals and "moderns" in the Levant.

Anyway the Levantine "soup" or basically Neanderthaloid population (if it was) was *not* the source of the Upper Paleolithic of that area or of Europe or western Asia. To put it differently the Cro-Magnon type modern human beings and their Upper Paleolithic (blades, especially) culture did not evolve in the Levant or come from another section of the Near East. The first culture of the "Upper Paleolithic revolution" was *Emiran* which does not seem to be descended from Levantine cultures. Later on (16,000 to 20,000 BP) Aurignacian from the Balkans came to the Levant temporarily during the Last Glacial Maximum.

Did the Emiran come from Morocco, where some early "moderns" have also been found about as early as Qafzeh? (My question) No, Ofer did not think so because the Moroccan sites in question are not well dated. They are probably nowhere near 100,000 years old.

The Upper Paleolithic blade industries and Cro-Magnon type modern human beings first came to the Levant around 47,000 years ago, perhaps a bit earlier but not later. This was *before* they showed up in Europe. They came from the *south*, probably from the same eastern African areas the first "moderns" had come from. There we have "upper paleolithic" type MSA cultures with blades around 57,000 or earlier. He quotes Stanley Ambrose to this effect. (I have a vague memory of Ofer saying or someone else saying that an African blade culture of 80 kya may have been involved.) (The reader will recall Henry Harpending using African sites to argue that "moderns" did not necessarily come from Africa. In MT-22)

Now Ofer is considering drawing the rest of the conclusion — that modern people spread around the rest of the world from Africa of (say) 60,000 BP. They very likely followed the same general routes previously followed by the several migrations of *Homo erectus*. He has to get them to Australia by 55 kya or possibly more recently and to a new site in eastern Siberia of 30 kya and, of course, to northeastern Asia or coastal Japan-Aleutians by probably 20 kya (conservative date) in order to have the ancestral Amerinds settle the New World. But I would guess that he has to get "moderns" to either India or mainland Southeast Asia earlier than 55 kya because the Australian cum Papuan leap across the sea requires some preliminary time of settling down in the vastness of Sundaland.

Well, logically we have a problem then, queries Fleming. What happened to Henry Harpending's nesting areas which we discussed in MT-21? Ofer said that Henry had changed his mind a little bit. He was now talking about *bottle necks*. Migrating populations passed through those and got



substantially altered. In effect Henry's nesting areas are not needed if we can just say that the bottle necks produced the distinctive clumpings of modern people, racially and linguistically. Perhaps. We have to check with Henry about his new interpretations. But I am sceptical.<sup>2</sup>

### Other Fronts

(1) Ofer's writings and comments on the subject of the Neolithic Revolution are authoritative, in the sense that he is recognized as an important scholar or expert on that subject. He strongly believes that the analysis of the Neolithic can be a model for thinking about the Upper Paleolithic Revolution. We might add the Language Revolution too, if we can figure where to connect that with *materia archeologica*.

The most likely epicenter for the agricultural revolution which occurred between 10,300 and 9900 BP or 8000 BC  $\pm$  was the Jordan river valley. Here it seems clear that there was a direct continuity from a nomadic hunting-gathering culture to a sedentary hunting-gathering culture to a sedentary farming culture — all in one relatively small place. It is associated with the early Natufians of 13,000 BP or 11,000 BC  $\pm$  who responded to a lush habitat ushered in by climatic change and settled down in stone houses. In a few millennia, the climate altered itself again, encouraging the systematic management of cereal grains, instead of the usual relapsing to nomadism. Voilà! the agricultural revolution! From there it spread in most directions and gathered speed when it hit the open plains of Anatolia. Domestication of sheep, goats and pigs followed after a while: 9500 to 8000 BP in various Near Eastern communities. Therein also, of course, the potential origins of pastoralism.

We have to interrupt to deal with some contrary data. The New York Times reported on May 31, 1994 that "First Settlers Domesticated Pigs Before Crops". Written by John Noble Wilford, the article stated that pigs were domesticated circa 10,400 to 10,000 BP at Hallan Cemi in extreme eastern Turkey, near the Batman river on the upper watershed of the Tigris. There were *no* evidences of grain cultivation or sheep or goats. The article's cited archeologists flirted with the notion of claiming that their Neolithic was earlier than or separate from that of the Levant. In any case, they had evidence of much slaughtering of young pigs plus evidence of changes in tusks which probably followed domestication.

So what do we do with domestication about 1000 years earlier than that of the Levant? The Bar-Yosef response was to say that he knew about the site and its excavators, but he could see only that Hallan Cemi was an intensification of the hunting phase of hunting-gathering. It was not the beginnings of the proper Neolithic revolution. There had been earlier claims of "premature" domestication of sheep in the Zagros too (I forgot the site's name), but Ofer said that had not paid off. Besides, he said, if they domesticated pigs at Hallan Cemi, "where is the pig shit? where are the coprolites which you would expect?" — that is if you had scads of pigs defecating in your village. So the search for porcine poops will commence!

(2) Ofer explained that there were two basically different cultural groups in the Near East during the Neolithic times, with roots probably going into a deeper past of differences. In the Levant, we had hunter-gatherers who became sedentary. One of their earlier stages was *Geometric Kebaran* which developed into the Natufian and later into Khiamian Sultanian (not everywhere). One of its key "lithic markers" (my coinage) was what McBurney used to call "trapezoids" or "trapezoidal points"; arrowheads and such.<sup>3</sup> That tradition contrasted with another sedentary hunter-gatherer culture associated with the upper basin of the Tigris or the great plains of Syria and northern Iraq. It had a kind of "lithic marker" of basically triangular points which were associated with *Zarzian* culture, which also had representatives in the Zagros and the Caspian area. One of its later manifestations was Hallan Cemi, the place of pigs.

(3) Ofer talked about the relative strengths and weaknesses of different schools of archeology.<sup>4</sup> The Anglo-American tradition has its strengths, of course. It has "gone high tech" and examines ecological data with very finely toothed combs. But there has been some slackening of interest in the analysis of stone tools. Ofer noted that European experts occasionally disagree with the Americans on the analysis of crude or indistinct stone tools (or are they geofacts?). Ofer is inclined to think the Europeans are probably right and that the American opposition to the pre-Clovis sites reflects their relatively low stress on lithic analysis. Just consider the Tasmanian upper paleolithic, says Ofer. It certainly looks crude and Oldowan-like in its stone tools which would be called lower paleolithic in Europe. But in their work on bones, the Tasmanians showed skill which was at an upper paleolithic level.

(4) Incidentally, in his manuscript, Ofer in effect brought back the logic at least of the old Movius Line (which we discussed way way back in MT-2 or MT-4) but with a new and arresting twist. It used to be Hallam Movius' proposal that Asia east of some point in India had a core tool or chopper/chopper tool tradition which has been likened to *adzes*, while west of that point in India there was the biface or *hand axe* tradition. Both are connected with *Homo erectus*. However, it now appears that the chopper tradition came out of Africa and settled "the world" of that time; it was first in the western half too. Later on, a different bunch of *Homo erectus* types, probably also from Africa, created the handaxe tradition and spread it around in *parts of the west*. Much of Europe and western Asia (northerly) remained in the chopper tradition. It probably follows that their local *Homo erectus* differed from the new Africans with their spiffy axes.

**Ex Africa semper nova.  
It's still true.**

(5) And our last observation, speaking of Africa, concerns the *Aterian*, an archeological industry of MSA type. Its distribution covered a very large area, maybe as big as Canada,

centered on the Sahara but touching Egypt and the Maghreb and the Sahelian margins to the south. Ofer's point with respect to the Aterian was that it was "out of it" in relation to the Mousterian and the Neanderthal vs "moderns" question. Essentially, it was something like a dead end which had developed out of the Mousterian as a specialized form. He would deny that Cro-Magnon or the Upper Paleolithic arose out of the Aterian.

So what does one make of the Aterian? It *might* be associated with the nesting areas of Niger-Congo or Nilo-Saharan. Or even Khoisan, if one remembers Carleton Coon's old idea that the Bushmen were to be called "Capoids" and that they were ultimately derived from northern Africa.

We solicit thoughts from colleagues on both Aterian and Coon's theory of the Capoids. Please do not, however, use his alleged racism as an argument. It's irrelevant, in the sense that his favoritism towards white people tells us little about the truth value of his hypotheses. Remember : even those out of fashion may utter the truth from time to time.

Our good colleague, Ofer Bar-Yosef, is now back from the field (Turkey, Georgia, Israel, etc.). Next he will be on sabbatical, having gotten an NSF grant. We doubt that his terrific energy will slacken. We are sure that we will be able to report some more of his thoughts — later.<sup>5</sup>

### Notes

1. So-called modern industrial or industrialized nations are analogous. Each has numerous cultural and social differences from the others. Within the overall typological similarity, however, most of them are "white" or European (in origin). Japan and Taiwan are different, being non-European physically and culturally.

2. Henry Harpending did change his mind a little bit. He still believes the main tenets of his paper are correct, to wit, neither out of Africa at one fell swoop with Eve as the mama, nor the rising tide lifts all boats or Weidenreich-Coonismus, are tenable.

Now Henry would modify the nesting area notion — which was actually my interpretation of his ideas — to propose basically one nesting area with a later dispersal of modern humans around the world. The prime nesting area was in Africa, probably eastern, and the dates would be in the 60,000 to 80,000 BP range. If this does not properly reflect Henry's current views, we will retract promptly! So Henry and Ofer are more or less in agreement, and out on the same limb together, but that makes them honorary astronomers!

3. It should be noted that McBurney's trapezoids were found in North Africa. It is tempting to associate this wider trapezoidal picture — do we mean horizon? — with Afroasiatic. One startling ethnographic feature of the modern world is the East African custom of bleeding cattle by shooting a special arrow into the carotid arteries of oxen or bulls. The arrow point is trapezoidal. While a handy functional explanation of this is that

such an arrow does not go all the through the artery but only punctures it, that does not preclude this being a very old custom. Murdock in his Africa book (and his student Fleming later) concluded that this bleeding of cattle was a very old Cushitic custom, later lent to immigrant Nilotes (e.g., Masai) and Bantu (e.g., Kikuyu).

4. (This footnote is Fleming's opinion:) Schools are not generally recognized in archeological theory but in fact there are often important differences in procedures and skills between scholars trained in different countries or even at different colleges. There are often different emphases — the "what everyone ought to be doing" type viewpoints — which make a great difference sometimes in *where* people look for antiquities and *what* kinds of finds they are preoccupied with. Hal remembers that for many years at his former university one could not distract archeologists from their pursuit of the Bantu Iron Age. (While Africanist linguists nowadays seem to have gone bananas over tones and ATRs or "advanced tongue roots".) None of these pursuits are detrimental to the growth of archeology (or African linguistics); they just show that "people are doing" different things at different periods and regions. We academics can be a terribly arrogant lot; many people have been told that their grants were not approved or that their teaching a particular course was not approved because "we don't do that anymore" or "get with it, man! everyone is looking for kangaroos these days!"

5. Forgotten point on the Natufian. We noted earlier that Shnirelman and Militarev have proposed the Natufians as the ancestral Afrasians. With Ofer's dates (Early Natufian) that would be about 13,000 years ago, with possible local sources going back to perhaps 18,000 or 19,000 years ago. I told Ofer that I had put Natufian down as the Afroasiatic homeland in my dissertation about 30 years ago but since then had changed my mind. Ofer politely but pointedly wanted to know just what was wrong with the Natufian as the source of Afrasian. My response was that it was possibly old enough but basically was too far north. If you consider how much of Afroasiatic lies in the Sahel or Ethiopia, you are bound to put your central dispersal area nearer to the mid-point of the distribution of the major branches. Three major branches (sub-phyla) of Afroasiatic lie south of Egypt, one extending as far south as central Tanzania. Moreover, the southern branches were "heavier" than the northern ones, i.e., their internal differentiation suggests very strongly that they have been living in their general areas for a long long time. Especially Omotic and Cushitic.

And now Beja has been reclassified by Hetzron (morphological evidence) and Fleming (lexical evidence) as a distinct branch. With Egyptian, Berber, and Chadic, it argues powerfully for a Saharan homeland. But Ongota, now formally proposed as a distinct branch of Afrasian, is only going to reinforce the weight of Omotic and Cushitic. Is it not yet obvious that Semitic is an Asian outlier, not the source of Afroasiatic?

## WONDERFUL NEW BOOK FOR PREHISTORIANS AND GLOSSOGENETICISTS

An important new book on archeology and physical anthropology, entitled *The Evolution and Dispersal of Modern Humans in Asia*, Takeru Akazawa, Kenichi Aoki, and Tasuku Kimura, eds., 1992, has come out (Hokusen-sha Printing Co., Japan). Although dated two years ago, it did not reach Harvard Library until this June. It is the report of a conference held in Japan in November, 1990. Nevertheless, the topics appear to be very up-to-date, perhaps because the authors had plenty of time to revise. However, there are panel discussions of great value which obviously had to be published as they had occurred in 1990.

Many good people gave papers, and the book is weighty in substance. While a proper book review is not in order here, I will give the abstracts of particular papers and sometimes some comments on the more detailed contents. It would be useful if people got their libraries to buy it. Yes, I am "plugging" it as Americans say, but it is not a shame to advocate the purchase of a fine piece of scholarship. The authors inside the volume do in fact disagree with each other quite often, so it is not a particular position that I am plugging here. The bibliographies themselves are almost worth the cost of the book. (Price unknown)

Geoffrey G. Pope. Pp. 3-14. "Replacement Versus Regionally Continuous Models: The Paleobehavioral and Fossil Evidence from East Asia."

"Neither the archeological nor paleomorphological evidence from the Far East supports the notion of the total replacement of *Homo erectus* or localized descendants of this species by an invading population of anatomically modern humans from Africa. Rather, it is possible to show that there is regional continuity in both the archeological and human paleontological record in Asia. Specifically, the archeological record of the last one million years provides no evidence of the rapid introduction of technological innovation or a major shift in adaptational or exploitative strategies. Recently recovered evidence strongly suggests that regional archeological differences within the Far East may date back to the earliest hominid colonization of the region and extend, only slightly altered, until well into the Late Pleistocene."

"Furthermore, the evidence from the facial morphology of fossil and extant Asian hominids suggests that certain facial characteristics, particularly in the mid-facial region, also unite early Asian *Homo erectus* and extant East Asian populations. Certain features, such as the *incisura malaris*, a horizontally oriented inferior cheek contour, an inferiorly situated zygomaticomaxillary root and vertically shortened maxillae root (which are also found in extant non-Asian populations) are not only found in higher frequencies in fossil and extant East Asians, but also seem to have originated in the Far East. Thus, at least part of the modern genome of

anatomically modern *Homo sapiens* originated in Asia. A more plausible scenario for the emergence of modern humans seems, from the archeological and anatomical evidence, to be one which incorporates migration and hybridization as the basis of the behavioral and morphological commonalities which characterize modern humans."

Anne-Marie Tillier. Pp. 15-28. "The Origins of Modern Humans in Southwest Asia: Ontogenetic Aspects."

"Study of the Skhul and Qafzeh immature individuals is a means of understanding the adult morphology of early anatomically modern humans. My aim is to focus on the anatomical regions in which clear differences are present between these hominids and other Mousterian hominids, such as Neandertals. Most of the data deal with cranial morphology. A related issue concerns the relative rates of development of the two separate populations. The assumption that the emergence of modern humans was accompanied by a retardation in growth is not supported by the data presented here."

The last paragraph of her conclusions follows (p. 24-5):

"The Skhul and Qafzeh immature individuals display a mosaic of features in ontogenesis: (1) juvenile features shared with all *Homo sapiens* children related to bone maturation; (2) primitive retentions which cannot be used as indicators of evolutionary continuity with Neandertals; (3) derived modern features which fundamentally separate them from Neandertals. The hypothesis that the differences in morphological patterns between the Skhul-Qafzeh immature individuals and Neandertals can be understood in terms of distinctive growth pattern is not supported. Documenting the Neandertal ontogenesis, growth processes resulting in distinctive Neandertal features are especially evident in the oldest individuals (i.e., Teshik Tash). Any assumptions that the Skhul and Qafzeh individuals needed additional time for the completion of growth relative to Neandertals require more substantial data."

Bernard Vandermeersch. Pp. 29-38. "The Near Eastern Hominids and the Origins of Modern Humans in Eurasia."

"The recent dating of Qafzeh and Skhul caves has shown that a morphologically modern human population (*Homo sapiens sapiens*) was living in the Levant 100,000 years ago. They possessed nearly all of the metric features and morphology of the Cro-Magnons of the European Upper Paleolithic. Only some minor features (the thickness of the tympanic bone, for example) show that the evolution of the modern human form had not as yet been fully achieved. It is reasonable to establish a phylogenetic relationship between these Levantine fossils and the Cro-Magnons.

"We may postulate that modern eastern Europeans, at least, originate from the Middle East. The great antiquity of the Aurignacian industries of eastern Europe (the Cro-Magnons are generally discovered associated with Aurignacian tools) constitutes an argument in favor of the hypothesis of the progressive peopling of Europe from east to west by the Cro-

Magnons, who replaced local Neandertal populations."

"For the moment, we have no evidence supporting the participation of proto-Cro-Magnons in the recent settlement of Asia."

"The origin of the proto-Cro-Magnons has not yet been established. They may derive from a local population of archaic *Homo sapiens*. The Zuttiyeh skull, which dates to about 150,000 BP or less, and which possesses no Neandertal features, may represent the ancestral population. There are no fossils more ancient in this region than the Zuttiyeh skull, and two hypotheses may be proposed:"

"1. *Homo sapiens* is very ancient in the Middle East and originates through *in situ* evolution from the first hominid populations in this area.

2. The populations from which the modern humans like Qafzeh and Skhul hominids derive arrived in the Middle East recently, perhaps 200,000 years ago. In this case, their origins are probably to be sought in Africa.

Whichever hypothesis proves to be correct, the Qafzeh and Skhul hominids are among the oldest fossils with modern morphology, and they play a very important role in the recent peopling of the Middle East and Europe."

From pages 39 to 47 there occurred a very interesting discussion of the first day's papers; it was chaired by Erik Trinkaus. Primarily, it was obsessed with the quantity and quality of cranial measurements taken on fossil skulls and the comparisons with each other and modern skulls. Some studies showed that Neandertals were distinctly different from both moderns and the Qafzeh skull but others essentially disagreed. Reading that discussion could be educational.

Sultan Muhesen. Pp. 51-65. "The Transitional Lower-Middle Paleolithic Industries in Syria."

"The Last Interpluvial period seems to have been very complex. Joint geomorphological and prehistoric research in Syria has shown that different, but contemporaneous, industries coexisted there and marked the Lower-Middle Paleolithic transition. Some of these industries were derived from an Acheulian origin. These came mainly from the valleys of Nahr el Kebir and Orontes. They are characterized by bifacial artifacts accompanied by Levallois débitage. Other industries are almost completely innovative and are represented by Pre-Aurignacian, Yabrudian, and Acheulo-Yabrudian, known previously from Yabrud (Rust 1950)."

"Recent work in the oasis of El Kowm in the Syrian desert has thrown more light on this problem. Many sites with an extraordinary abundance of archeological material were associated with springs, the majority of which are now fossilized. Hummalian is an original industry coming from a geological level of loose sand preceding the Mousterian in Bir el Hummal. It is characterized by a blade industry resembling the pre-Aurignacian and the Amudian. The Yabrudian was found just under the Hummalian in conglomerating travertines and was dated to around 150,000 BP (Henning and Hours 1982)."

"The current data may suggest a continuous sequence

originating from an evolved Late Acheulian to a series of these different contemporaneous industries, but we cannot tell how this continuum relates to the fossil human record. We are also not yet able to give any definitive interpretation of the causes of this phenomenon, although cultural, economic, ecological and other factors can be taken into consideration. Nor are we able to precisely explain the Mousterian domination at the beginning of the Last Pluvial."

His last two paragraphs sum up his fascinating paper, thusly:

"So we can agree with Bar-Yosef (1982) that the transition is characterized by a continuum and that the innovations were gradual without causing a cultural break between the Lower and Middle Paleolithic."

"Thus we are able to establish cultural continuity on the basis of the archeological evidence, but have less information on the human fossils, which are very rare and, up to now, only found in Palestine [or Israel - HF]. The fragmentary skull of Zuttiyeh is considered to come from an "Acheulian or Yabrudian facies level" (Gisis and Bar-Yosef 1974) and it is dated 148,000-90,000 BP (Schwarcz et al. 1980). Thus Yabrudian is the only Lower Paleolithic culture which yielded human remains. The Mousterian layers at Qafzeh, which contained human fossils, are dated to about 100,000 BP by thermoluminescence (Valladas et al. 1988; Vandermeersch and Bar-Yosef 1988). The Zuttiyeh man is considered to be an early archaic *Homo sapiens* (Vandermeersch 1982). He was followed by the proto-Cro-Magnon of Qafzeh (Vandermeersch 1981, 1982; Bar-Yosef et al. 1986). Thus continuous human evolution from Zuttiyeh to Qafzeh is suggested (Trinkaus 1982)."

"If we accept this conclusion, the transitional industries were produced by an early *Homo sapiens sapiens* (or, just as likely, by an evolved *Homo erectus*). In this case *Homo sapiens sapiens* of the Levant is earlier than the Neandertal, as is indicated by the Kebara burial dated to about 60,000 BP (Valladas et al. 1987). But, for a certain time, the two hominids were contemporary and had cultural affinities, not only in stone-knapping but in their funeral rites as well (Tillier et al. 1988). We have to await, however, further archeological and anthropological evidence to verify this point."

Naama Goren-Inbar. Pp. 67-82. "The Acheulian Site of Gesher Benot Ya'aqov: An African or Asian Entity?"

"Since its discovery the Acheulian site of Gesher Benot Ya'aqov (northern Jordan Valley, Israel) has been considered an African entity. This view was based on typological and technological characteristics of the lithic assemblage as well as on the geographical location of the site in the Dead Sea - East African Red Sea Rift System."

"The recent discovery of previously unknown Middle Pleistocene outcrops assigned to the Benot Ya'aqov Formation initiated the current interdisciplinary research."

"This article re-examines the 'African' characteristics of the site, as expressed in typology, technology and the mode of raw material exploitation. The latter shows a pattern specific

and common to the Lower Paleolithic sites of the Dead Sea Rift. While various typological and technological characteristics do appear both in Africa and the Levant, the lack of radiometric data does not allow us to assign them or to attribute their first appearance to that area. Nevertheless, the Gesher Benot Ya'aqov archeological horizons and occupations are definitely closer to the African Acheulian sites than to any other entity."

"Thus Gesher Benot Ya'aqov cannot be strictly assigned to either an African or an Asian entity; it is a unique, independent phenomenon, having paleoecological characteristics and encompassing various traits, of which many are local and few can be assigned to a "foreign" origin or influence."

Note to non-specialists: the GBY site is not directly pertinent to *Homo sapiens* in Israel but could theoretically be the industry out of which the Mousterian found at Qafzeh and elsewhere was derived. The "Levallois technique" is found both at GBY and in Levantine Mousterian. What does it prove?

Katsuhiko Ohnuma. Pp. 83-106. "The Significance of Layer B (Square 8-19) of the Amud Cave (Israel) in the Levantine Levallois-Mousterian: A Technological Study."

"The lithic materials from Layers B4 and B2 of the Amud Cave are placed in a proposed variant or phase of the Levantine Mousterian modelled by Tabun B. In spite of the similarity between the two materials in their overall artifactual features, there are sufficient technological differences to separate them into two variations. The industrial variability proposed for the Amud materials may increase the importance of the study of the Levantine Middle Paleolithic, adding archeological data to anthropological research which seeks to clarify the evolutionary processes of modern humans in the region."

Note: the title is misleading. This is not early Mousterian but rather "transitional industries between the Middle and Upper Paleolithic of the Levant", dated from 18,300 ± 400 BP in the base to 5710 ± 80 BP in upper levels, i.e., not so long before the Neolithic to well into it. Ohnuma seems not to realize this.

Yoshihiro Nishiaki and Lorraine Copeland. Pp. 107-127. "Keoue Cave, Northern Lebanon, and Its Place in the Context of the Levantine Mousterian."

"Keoue is a Levantine Mousterian cave site in northwestern Lebanon, the only one to have been excavated and to have produced a good artifact sample. A techno-typological analysis (previously unpublished) of the lithics is presented, the results suggesting that the Keoue industry refers to the Tabun B-type facies, as known from Garrod's Tabun B material. On this part of the coast, several stratified Middle Paleolithic cave and open sites show that the B-type facies occurs in Würmian (i.e., Late Mousterian) contexts, overlying Tabun C-type facies associated with Last Interglacial (Enfean) raised beaches, thus demonstrating the sequence of B-type over C-type here."

"The variability of Mousterian facies, as it pertains to

the evident differences between the north and south Levant, is discussed, and Keoue is dated, based on the recently published dates for a B-type facies at Kebara, to between 70,000-60,000 and 45,000 BP."

"No human fossils were found at Keoue, but the corpus of specimens from the northern region (Masloukh, Ras el-Kelb I-III, and Ksar Akil I and II) are reviewed. Judging by the incomplete evidence, Keoue may have been a Neandertal site."

Note: since Lebanon is a Francophone area, Keoue would be [kewe] in IPA.

Liliane Meighen and Ofer Bar-Yosef. Pp. 129-148. "Middle Paleolithic Lithic Variability in Kebara Cave, Mount Carmel, Israel."

"This paper discusses the dynamic process of Levallois core reduction systems in order to reveal the choices made by Middle Paleolithic humans, beginning with the selection and transportation of raw material, until the abandonment of the utilized objects. This process, known in French as the '*chaîne opératoire*' (operational sequence) reflects the flintknapping behaviors of prehistoric artisans. The analysis includes observations concerning the differential selection of raw material, methods used for the knapping, selection of blanks, and retouching and resharpening of chosen pieces. The lithic assemblages from the Mousterian layers in Kebara serve as detailed examples. Core reduction strategy in Kebara is characterized by the recurrent Levallois production method, through which numerous flakes, as well as blades or points, have been struck from the same flaking surface. In the upper units (VII and VIII) the core reduction is oriented towards the production of subtriangular and quadrangular flakes, Units IX and X it is marked by the production of short, broad-based Levallois points with protruding striking-platforms ('*chapeau de gendarme*' butts), while in the lower levels (IX to XII) it is characterized by a slight increase in elongated blanks, often of triangular shape. Comparisons with other sites disclose the similarities between Kebara and Amud, Ksar Akil Layer XXVIII and Tabun B. Qafzeh and Tabun C present a distinct character while the Tabun D industry only superficially resembles Kebara Unit XII-XI. The core reduction strategy employed in Tabun D for the production of elongated blanks, mainly pointed blades, differs considerably from the method used in Kebara. It therefore seems that the current analysis supports the contention that at least three different industries can be recognized among the excavated Mousterian assemblages in the Levant."

Two of their conclusions are given here:

"If the hypothesis concerning the ability of human groups to be characterized by their technical habits is correct, then this study constitutes a step in that direction. The sequence of tool production discussed in this paper for all the Kebara layers indicates that the unidirectional Levallois technical system, which is widespread in the Near East, stands in contrast



with the dominant core reduction strategies in nearby regions such as Egypt, Nubia and Libya as shown by Crew (1975a, 1975b). It is even more distinct when compared to the common methods used during the Würmian Middle Paleolithic of western Europe."

"We have demonstrated that assemblages in which tool manufacturing methods are similar to those from Kebara and Tabun IX can be compared with each other and are clearly distinguishable from those dominated by the radial method such as Tabun C and Qafzeh. We therefore believe that on the basis of these observations there is no point in grouping Tabun B and C into one phase, as the two industries differ considerably in their technologies. The question that seems to us more pertinent is to what extent the assemblages from Tabun B and D can be differentiated, and what these differences might mean."

Eitan Tchernov. Pp. 149-188. "Biochronology, Paleoecology, and Dispersal of Hominids in the Southern Levant."

"Ever since the Early Neogene, the Levantine region has been biogeographically situated on the main corridor between Africa and Eurasia. Each biotic exchange that took place between the two realms, have used the 'Levantine Corridor' as a major passageway. Indeed, the Levant as a playground for northward-southward shifting of the Palearctic and Afrotropical zones across the Saharo-Arabian arid belt, intermittently introduced European and African taxa into the region in correlation with Pleistocene climatic fluctuations. Expansions and emigrations of hominids, mostly of Afrotropical origin, played an integral part in biotic dispersal events until the late Upper Pleistocene. Being basically adapted to open habitats, dispersals of hominids were always associated with open country biota."

"The 'Ubeidiya Formation' (Jordan Valley, Israel), dated at present to 1.4 million years ago, may actually represent one of the oldest known *Homo erectus* sites outside Africa. This early exodus from Africa was associated with a large-scale invasion of East African mammals. Evidence for a later large-scale expansion of early hominids to Eurasia indicates a Middle Acheulian episode, recorded from Gesher Benot Ya'aqov (Jordan Valley), that once again was associated with the introduction of new Ethiopian elements into the region." [Ethiopia = sub-Saharan Africa in this context - HF]

"More controversial is the earliest dispersal of modern humans from tropical Africa to the Levant. This event was evidenced in the cave of Qafzeh (Israel) where an archaic community of micromammals was recorded, and on the basis of the local biochronological sequence, was dated to the Last Interglacial, or to late Stage 5. TL and ESR datings of the bone burial bearing beds of Qafzeh have shown an age of 90,000-100,000 BP. The Qafzeh population of anatomically modern humans is associated with Afro-Arabian elements. If the skull of Zuttiyeh (Wade Amud, Israel) is classified as a later form of *Homo erectus*, then the first colonization of modern humans in Eurasia took place within a *H. erectus* domain, and no Neandertals existed during this period in this region. The early expansion of modern humans ("proto-Cro-Magnon") was

confined to the Levantine region. The transition from Stage 5 to 4, as evidenced in several Mousterian deposits (Unit II of Tabun D, and/or Tabun C, Hayonim Lower E) was distinguished by rapid biotic change, caused by abrupt invasion and predominance of Palearctic elements in the southern Levant. This invasion was associated with the first introduction of Neandertals into Southwest Asia. Updated information on Middle Paleolithic humans in this region does not show sympatry between the two forms. It is hence assumed that during Stage 4 Neandertals kept modern humans back in Africa or the Saharo-Arabian belt, and their pattern of distribution remained essentially parapatric. The last and abrupt large-scale and terminal invasion of modern humans to Eurasia through the Levantine corridor took place in the early Upper Paleolithic, yet this time it was not associated with any biotic dispersal. It seems that during this period human dispersals already began to be completely detached from ecological factors, and became more dependent on socio-economic and cultural necessities."

Note: Professor Tchernov is at Hebrew University of Jerusalem, Kefar Etzion Street 19, Arnona, Jerusalem 93 392, Israel — just in case you want a reprint of this nice article.

Ofer Bar-Yosef. Pp. 189-215. "Middle Paleolithic Human Adaptations in the Mediterranean Levant."

"The aim of this paper is to discuss the differential success of the late Middle Pleistocene and Upper Pleistocene hominids in western Asia. This will be done on the basis of an updated summary of the available information concerning chrono-stratigraphy, faunal spectra and site locations. Environmental reconstructions will serve as a basis for assessing the nature of the ecozone exploited by hominids. Stratigraphic gaps, changes of site size and of lithic assemblages are employed in order to estimate the intensity of human occupations within small territories as a measure for the degree of mobility, population densities, and survival strategies. This paper, therefore, departs from basic archeological and paleo-environmental observations and enters into the realm of interpretations."

"The scale with which the success and failure of human societies is measured is based on data sets collected in marginal zones (cold and warm deserts). Detailed information is available for western Europe, and fragmentary sequences are recorded for the eastern Sahara and the Levantine deserts. The major Levantine paleoclimatic events, tentatively correlated with the isotope stages, are briefly summarized as a background for evaluating trends in human adaptations. However, the short-term climatic fluctuations that directly influence human subsistence strategies are poorly known due to the gross-grain chronological resolution. Moreover, uncertainties concerning the amount of behavioral shifts among the western Asian mammals reduce the potentials for environmental reconstructions."

"In a cautious estimate of past changes which took place during the past 200,000 years, the Levant is seen both as a corridor for movements of humans and animals and as a refugium during climatically harsh periods. The mixture of

morphological characteristics of the available Middle Paleolithic human fossils are interpreted as the result of constant gene flow between incoming and locally survived groups. The marked change in successful adaptation among Upper Paleolithic humans is attributed to both biological and technological changes."

He begins with a very instructive quote from F. C. Howell (1952); especially for non-archeologists it will help explain some of the puzzling variability of ancient Palestine. It goes:

"Broad areas of Europe and Asia during the glacial periods, though tolerable for specially adapted arctic birds and mammals, were ill-fitted for permanent human habitation. Regions such as western and southern Europe, which were a continuous part of the temperate sub-tropical continent during the inter-glacial periods, became, during the glacial advances, peripheral refuge areas where, though climatic conditions may have not been too congenial, there was nevertheless loss of contact with other human groups because of the surrounding climatic barriers."

He then adds some thoughts of his own to the general problem: (p. 190)

"The topic of human adaptation is worth a systematic discussion because at least some human groups made it to the Late Pleistocene period when most ecological niches became occupied. One may wonder why it took the human stock such a long period of time to colonize the entire globe. On a geological scale, the last 2.5 million years cannot be considered as a long period. During this time bipedal humans gathered vegetal foodstuffs, scavenged preyed animals, made stone tools and became hunter and finally farmers. The rate of change is not constant and in spite of the incompleteness of the archeological record and uncertainties involved in the published reports, one may cautiously state that by 1.4-1.0 million years ago humans moved out of Africa, by 40,000 years ago colonized Australia and by 20,000-15,000 years ago penetrated into the New World. The slow change during the Lower and Middle Paleolithic dispersals across the African and Eurasian continents needs some explanation. Proposed hypotheses should take into account possible numerous extinctions which are reflected in archeological gaps. This paper aims to discuss the course of environmental changes during the last 200,000 years in the Mediterranean Levant as a background for Middle and Upper Paleolithic adaptations."

Discussing adaptations to Eurasian environments later (p. 191-192), he continues:

"Under the periods of increased precipitation during the Upper Pleistocene many inland basins accommodated small and large lakes, often brackish or salty. These environments provided adequate food resources which could have been used by Middle Paleolithic humans. By scavenging and gathering,

the survival of the social unit could have been secured. Long term stress periods were those when the climate became cold and dry, lakes shrank, numerous springs dried out and the mammalian communities were considerably reduced due to death and migration. The advantages of the Levant is that the coastal ranges could be exploited year round under any circumstances. Therefore local expansion and retraction of populations is expected to have taken place. However, within a wider framework, human groups could migrate into the region from Northeast Africa during wetter periods when desert biomass increased considerably. Southward movements from Anatolia (or the Taurus and part of the Caucasus/Zagros ranges) became feasible when glacial conditions made the Anatolian plateau and the higher altitudes of these mountain ranges inhospitable. Finally, there is no positive evidence which would indicate that the main pattern of seasonality was entirely different from today's. The total vegetational assemblage of the Levant was basically the same during the late Middle and Upper Pleistocene as demonstrated by the various pollen cores, although one may expect invasions and disappearances of a few species that originate in the northern latitudes."

"The contention of this paper is that in spite of the presumed long-term experience in subsisting in higher latitudes shared by Eurasian hominids, the available technologies did not allow successful survival under full glacial conditions in periglacial environments and their ecotone with the temperate belt. The most stressful periods were when extreme cold conditions prevailed, notably during Isotope Stages 4 and 2. In the deep-sea cores, Stage 4 exhibits similar climatic conditions to those of Stage 2 (e.g., Martinson et al. 1987) although it was somewhat less harsh than the latter. I have used the published information concerning the conditions during Stage 2 in the Levant as a basis for reconstructing Stage 4 (Bar-Yosef 1988, 1989a, 1989b). If this procedure is valid, then Stage 4 was cold and dry and not wet beyond the coastal Mediterranean ranges."

"Finally, the nutritional and physical stress which results from a succession of cold and relatively dry winters in the northern hemisphere on population structure should be taken into account. General stress and especially food shortages often lead to diminishing numbers of females (Groëman van Waatereinge 1988) and a population decrease. For foragers whose environment became marginal it will be advantageous to move out. Delayed or wrong decisions lead to extinctions. To draw a picture of the societal evolution of the Lower and Middle Paleolithic without considering the extinction of human groups means ignoring the lessons of mammalian evolution. Without major technological innovations involved in food acquisition, which for the time being cannot be traced in the archeological record of most of the Lower and Middle Paleolithic, emigrations, migrations and extinctions are often reflected in gaps within archeological stratigraphies."

Ofer paints a broad picture of the archeological sequence in the Levant, along with scores of dates for particular strata (see his Table 11.1 on pages 194-196). (My uninformed opinion is that the C-14 dates seem to reach their limits here, lagging behind the TL and ESR dates as much as 40,000 years.



It would appear to be the reason that the modern humans were misinterpreted before; they were thought to be contemporaries of the Neanderthals while they actually preceded them by many thousands of years.) Ofer's rough sequence is, as follows: (not directly quoted for the most part, rather summarized):

The Upper Acheulian and the Acheulo-Yabrudian 220-140 kya

The second is also called the Mugharan tradition, its three facies are the Yabrudian, the Acheulian, and the Amudian. These basically underlie the Mousterian although in places they occur in the same periods. These facies differ in the number of bifaces (probably handaxes) they contain and the amount of more advanced tools like flakes, blades, and end scrapers. Also at El-Kowm in eastern Syria another industry called Hummalian was found. It has many blades and points but no Levallois technique. It is the stratigraphic mate to Tabun D. This whole lot underlies the next set but is not necessarily ancestral to it or the source of it.

The Levantine Mousterian which is our main focus is usually sub-divided by the strata found at Tabun. These have been rather intensively studied. Quoting now from pages 193-197:

"1. Tabun D - the blanks, blades and elongated points were predominantly removed from Levallois unipolar convergent cores with minimal preparations of the striking platforms. Elongated retouched points, numerous blades, racloirs and burins are among the common tool types. Rare bifaces occur. This industry is found at Tabun D, Abu Sif, Sahba, Rosh Ein Mor, Nahal Aqev 3, Jerf 'Ajla and Douara Layer IV." (Average date at Tabun is  $122 \pm 20k$ )

"2. Tabun C - the blanks, often ovaloid, and large flakes were struck from Levallois cores with radial preparation or the "lineal method" by Boëda's (1986, 1988) definitions. Triangular points appear in small numbers. This industry is common in Qafzeh, Skhul, Tabun Layer C, Naamé and Ras el-Kelb." (85-119kya)

"3. Tabun B - the blanks were removed from mainly unipolar convergent Levallois cores. Broad based point, often short, thin flakes and some blades (all made by the same recurrent method) (Boëda 1986, 1988) are common. The best examples for this industry are Kebara Units VI-XII, Tabun B and Bezez B. Radially prepared cores were used and their products are mainly found in the upper contexts of this entity (e.g., Kebara VIII-VII, Biqat Quneitra; Meignen and Bar-Yosef 1989; Goren-Inbar 1990)."

(These range from 45-96 kya - HF)

I have added italics in the next passage because of the significance of the remarks. They are not in the original.

"The Levantine Mousterian *differs markedly* from the Mousterian facies in the *Zagros and Taurus mountains*, to the north, although the Karain industry has a strong component of radial Levallois preparation (Dibble 1984; Yalçinkaya 1988). Mousterian industries which *partially resemble* those of the Levant can be found in the *Middle Stone Age of South Africa, Egypt and Cyrenaica*."

"The Upper Paleolithic: The Transition"

[Note: because of its importance, the whole section is reproduced here, with permission of the editors - HF]

"If cultural changes as reflected in lithic industries were gradual we should be able to observe, at least in stratified sites, a constant shift over a long time from the use of Levallois methods to prismatic core reduction strategies. But this is not the case as two, by-now famous Levantine sites, demonstrate."

"The earliest Upper Paleolithic industries from Ksar Akil, in the Lebanese coastal hilly ridge, and Boker Tachtit, in the Negev highlands, differ in size and quality of raw material as well as conditions of preservation. The assemblages from Boker Tachtit (Marks 1983) were recovered from distinct archeological horizons. The collections from Ksar Akil were obtained during excavations in 1937-1938 and 1944-1948 (Azoury 1986; Ohnuma 1989). Results from the lithic studies enabled Copeland (1975) to describe the transition from Mousterian to Upper Paleolithic as characterized by the continuation of the unipolar Levallois core reduction strategy. The blanks which were originally shaped into racloirs and points were later formed into end-scrapers, chamfered blades and flakes as well as burins. Re-analysis of the collections by Marks and Volkman (1986) demonstrated that the topmost assemblages are dominated by more radial preparation and thus casts doubts on a direct technological continuity."

"Boker Tachtit was shown by Marks (1983a, 1983b) to demonstrate the gradual shift which designates the onset of the Upper Paleolithic, from bipolar blade and Levallois core reduction to dominance of unipolar blade core reduction strategy. The earlier tool kit of the Level I assemblage is, in the European terminology, fully Upper Paleolithic. It includes Emireh points, Levallois points, end-scrapers, burins, etc. . . , but the quantitatively prevailing types are those of the Upper Paleolithic."

"Thus in both Ksar Akil and Boker Tachtit the major visible shift is first typological and only later in lithic technology. This observation may support the claim for autochthonous change. Similar techno-typological shifts were observed in western Europe in the sequence of Mousterian of Acheulian Tradition and the Castelperronian assemblages, but there it seems to be more the result of interaction between local groups and incoming populations. The reason for the different explanations concerning western Europe and the Levant is the long tradition of blade production which can be traced in the Levantine Middle Paleolithic. Moreover, while the West European Aurignacian differs considerably from the Castelperronian, the Levantine sequence presents an unbroken continuity from the transitional industry into the Ahmarian."

"The biological interpretation of the archeological transition is still questionable. A few scholars believe that the European sequence demonstrates biological continuity while others see groups of modern humans moving into this continent from the east. Those who hold the latter viewpoint may adopt a similar interpretation for the Levantine sequence. Thus, if this is accepted, we should look for the origins of Upper Paleolithic

human beings in a different region. For the supporters of the African origins hypothesis, this is a plausible notion. The currently published dates of the Howieson's Poort as 75,000-45,000 years from Border Cave (Grün et al. 1990) can be interpreted as major support for this view, although the age of Middle Stone Age IV from that site means a return to Middle Paleolithic lithic technology from 45,000 through 35,000 years ago."

"The Upper Paleolithic sequence which follows the initial transitional phase is characterized by blade/bladelet industries recently named the Ahmarian Tradition (Gilead 1981; Marks 1981) dated to about 34,000/28,000 through 22,000 BP. What seems to be a continuous sequence is interrupted by a different industry commonly referred to as "Levantine Aurignacian". The sites where this industry was found are mainly limited to the coastal northern and southern Levant. The Levantine Aurignacian B assemblages (Copeland 1975) are characterized by the dominance of flake production, nosed and carinated scrapers and El-Wad points. The presence of split-base points in two sites together with the proliferation of bone and antler objects hints to a source in the northern European world. The Levantine Aurignacian was subsequently followed by other flake industries (Gilead 1981; Marks 1981; Bar-Yosef and Belfer-Cohen 1988)."

"The Ahmarian blade/bladelet assemblages dominate the later Levantine Upper Paleolithic sequence, especially 22,000 BP onwards and their Terminal Pleistocene expressions are the various Epi-Paleolithic cultures (e.g., Bar-Yosef and Belfer-Cohen 1989; Henry 1989; Bar-Yosef 1990)."

In his concluding section, in discussing plant remains as evidence of gathering as opposed to the "cutting of animal tissue" which evidences hunting, he draws an important inference, to wit, "No doubt many more species were exploited but unless a very dry cave site or a water-logged open air site are excavated, we will have to concentrate on animal bones as the source of dietary information. This bias has additional implications; it puts aside the *discussion of the role of women* in Middle Paleolithic society." [My emphasis added; none in original text. HF]

Avraham Ronen. Pp. 217-228. "The Emergence of Blade Technology: Cultural Affinities"

"The assemblages known as Pre-Aurignacian, Amudian and Howieson's Poort date within Isotope Stage 5 or even 6, yet they share a closer similarity to the Upper Paleolithic/Late Stone Age than to the lithic assemblages which directly precede or succeed them. The similarities are seen in the typology, technology and subsistence patterns recorded in these assemblages. It is suggested that these occurrences represent the oldest archeological evidence for modern-type behavior. Molecular biology points towards Africa as the origin of anatomically modern humans, but modern behavior patterns first appeared in the archeological record of the Near East."

[Note: the range of dates suggested here is from 45k to as much as 182k, following Bar-Yosef's Table 11.1, but Ronen

never really clarifies his dates. His lithic analysis depends in part on François Bordes, a famous French expert on stone tools. I remain sceptical of Ronen's point. HF]

Anthony E. Marks. Pp. 229-251. "Upper Pleistocene Archeology and the Origins of Modern Man: A View from the Levant and Adjacent Areas."

"Two recent developments have profoundly affected thinking about the origins of modern humans: the claim that all of us ultimately derive from a single African ancestor and that early modern man was present in the Levant by ca. 92,000 BP. These claims have obvious implications for the interpretation of the archaeological record of both the Levant and adjacent areas of Africa, but to date, this aspect of the problem has not been addressed in detail. This paper is a first step in that direction."

"A careful view of the period 150,000 to 50,000 BP in the Levant shows evidence for both cultural continuities and disjunctions. The two major disjunctions, the appearance of the Mugharan Tradition and the Tabun B-type Levantine Mousterian, can reasonably be traced to movements of peoples from the north and east into the Levant both because of similarities in those directions and the absence of similar contemporary or earlier manifestations in Northeast Africa."

"In fact, an examination of the archeological record of Egypt and Sudan indicates that at no point in this long period is there any convincing evidence for Northeast African/Levantine connections on more than the technocomplex level. Therefore, it is concluded that the present archaeological evidence does not support an "out of Africa" model for the origins of early modern man in the Levant."

[Note: Marks is at Southern Methodist University, Dallas, Texas 75275 USA. His address was missing in the book's list of participants, so we give it here for those who may want a reprint of a strong paper. HF]

Marks' review of the North African and Egyptian evidence is useful and pertinent. His criticism of Bar-Yosef's and Tchernov's basic hypothesis is pointed; their sequences are wrong, the dates are less than certain, and the interpretation should be different. His tone is polite and reasoned. We need to look at his powerful and clearly expressed disagreement. The issue, of course, is of great importance to all long rangers. We will not settle it here.

His alternative interpretation is presented in Fig.13.3 on page 238. We cannot reproduce it but we can describe it in words.

First, the Upper or Late Acheulian is separated from the Mugharan Tradition. From around 150 kya, the Late Acheulian is native to the interior; it continues on until ca. 100 kya when it probably develops into Hummalian.

Second, the Hummalian probably develops into the Levantine Mousterian, generating both Tabun C and Tabun D, which he sees as essentially *contemporaneous*. They come to dominate the whole Levant from say 90 kya to 75 kya.

Third, the Mugharan Tradition (recall it was Yabrudian, Amudian, and "Acheulian") enters the Levant on

the north coast around 145 kya and dominates the whole Levantine coast for about 50,000 years. It never dominates the interior and in fact co-exists with Late Acheulian all the time. The Mugharan Tradition is *intrusive* from the north or east. If it is associated with any form of hominid, it is neither Neanderthal nor modern nor even archaic modern. We might call that evolved non-specific late *Homo erectus*. Europe has such too, e.g., Swanscombe man.

Third, the Mugharan Tradition is replaced on the coasts by the Levantine Mousterian ca. 100-110 kya. It sired nought, it seems, and became extinct.

Fourth, around 70 kya Tabun B-type Mousterian appears on the north coast and spreads to the south, never reaching the interior, or at least not all of it. Marks agrees that Tabun B is connected with Neanderthal, which was either intrusive from the north or had been there a while. His text puts the date as far back as 75 kya.

Fifth, Tabun B-type and its Neanderthals lasted to perhaps 48 kya when they were replaced by the Upper Paleolithic and its presumed modern humans.

Sixth, the transition to the Upper Paleolithic began 50 kya, later becoming the Ahmarian Tradition which dominated the Levant until the Neolithic and the transitions to it ca. 20 kya. This Upper Paleolithic shows up first in the "Marginal Zones" which I have been calling the interior herein. It is *autochthonous* and derived from *Tabun D*, not from *Tabun C*! Its roots go back to Hummalian and before that to the Late or Upper Acheulian.

Seventh, since neither Tabun D nor Tabun C are associated with Neanderthals, but Tabun C is connected to modern humans, then modern humans probably developed out of Palestinians of yore rather than immigrant Africans. [This is my translation of Marks. HF]

After an excellent review of the literature on the Nile Valley and North Africa, Marks fails to find enough similarity between African cultures of Africa and the Levant to justify calling either one the source of the other. However, it can be pointed out that on his map of type sites ("distribution of the major industries") on page 231, he lists C in the Levant and C in Egypt/Sudan (around Wadi Halfa, Asyut, and Bir Tarfawi). In the Levant C = Type C Mousterian, while in Egypt/Sudan C = "method group C". It is presumed that he means them to be distinct, but it is somewhat confusing. Indeed on page 242 in his "Tripole graph", the Egypt/Sudan C is the closest to Levantine Mousterian C which in turn is closest to its successor B. The tripole graph is quite interesting as a triangular classification by Bordes types most associated with Middle Paleolithic, Upper Paleolithic, and "denticulates". So maybe Marks himself has actually shown how the Nile Valley could be a source of the Levantine Mousterian C — again this is the one with the *Homo sapiens sapiens* in it.

A final point here would be to stress the dangers of negative data in archeology. Huge areas have barely been touched in Africa, especially the Red Sea hills, southern Sudan and almost all of western Ethiopia. I can remember when archeology stubbornly refused to accept many hypotheses about

farming and cattle in Africa, even when biological, ethnographic, and linguistic evidence strongly suggested them. Arguing from the absence of something is much more plausible in intensively excavated Europe or Israel than it is in poorly known Africa.

Arthur J. Jelinek. Pp. 253-275. "Problems in the Chronology of the Middle Paleolithic and the First Appearance of Early Modern *Homo sapiens* in Southwest Asia"

"The problem of the chronological relationships of particular hominid fossils and of distinctive lithic industries during the time period from the late Middle Pleistocene up to about 30,000 years ago has been a central focus of paleoanthropology for at least four decades. The relative abundance of fossil and industrial evidence in the Levant and its proximity to Africa and Europe has resulted in an especially intense interest in this problem in that region."

"When the stratigraphic archeological and paleoenvironmental record from the Levant, in conjunction with the global evidence of climate change reflected in the O-18 deep-sea record, is compared with the evidence thus far presented for absolute chronological relationships of hominid fossils and lithic industries, many serious inconsistencies are apparent. This comparison shows that it is highly improbable that many of the current age estimates based on thermoluminescence, electron-spin-resonance, and uranium decay series accurately reflect the absolute ages of the contexts from which they have been derived. While it is likely that some of the dates are accurate, it is not possible to sort them out on the basis of the current evidence. "Confirming" evidence based on two or three techniques that are vulnerable to the same causes of error may be particularly misleading in this regard. It will be necessary to establish better controls over the variables that affect the geophysical processes upon which these techniques are based, and to improve the resolution of the techniques before it is possible to establish a reliable absolute chronology."

[This is equivalent to throwing a bucket of cold water on Marks and Bar-Yosef and others - HF] There are two things to say about this: (a) dating is absolutely critical for prehistory but especially for archeology and (b) there seems to be a kind of central tendency to the many dates given by Bar-Yosef in his Table 11.1, which is encouraging. Some dates are clearly aberrant, particularly carbon 14 dates, and some are probably off, most likely the so-called "linear uptake" series of ESR. However, no serious dispute with Jelinek's main thesis is possible; the wild range of dates is apparent in Table 11.1.

Erik Trinkaus. Pp. 277-294. "Morphological Contrasts between the Near Eastern Qafzeh-Skhul and Late Archaic Human Samples: Grounds for a Behavioral Difference?"

"The documented presence of early modern humans in Middle Paleolithic levels at the Near Eastern sites of Qafzeh and Skhul, in addition to the presence of late archaic humans in similar Middle Paleolithic levels at the Near Eastern sites of Amud, Kebara, Shanidar and Tabun, presents difficulties in

interpreting the evolutionary biology of these apparently contemporaneous or alternating hominid groups. Namely, how did they manage to remain separate if, as archeological analyses currently indicate, their behavioral repertoires were essentially indistinguishable? Analysis of skeletal features which are known to respond to the behavioral patterns of individuals during development and adulthood, including dental attrition, scapular breadth, radial curvature, ulnar articular and femoral neck orientation, femoral shaft robusticity and shape, and gluteal tuberosity size, demonstrate that there must have been significant behavioral differences between these two Near Eastern Pleistocene hominid groups. These differences are likely, however, to live in behavioral components poorly reflected in current data on the techno-typological attributes of their lithic industries or species lists of exploited biota."

[Note: Trinkaus has zeroed in on the most perplexing part of the discoveries in the Levant. How did these guys co-exist and how come they are so culturally similar yet differ biologically? Underneath this is a further question about the whole Mousterian horizon on the "Western front": did it spread from its source region to some other, thus acculturating biologically different groups to itself, or was it more like the spread of some useful technology across different regions? (For example, bows and arrows) Trinkaus also reminded us that the problem with what to do about the two sets of humans in the Levant has been with us for half a century. Original decisions by McCown and Keith were crucial in "forever confusing our perceptions of Middle Paleolithic hominid evolution in the Near East." - HF]

In the next chapter, entitled *Second Day's Discussion*, chaired by Bernard Vandermeersch and Anthony E. Marks, the issues raised in various papers are debated. It is a fascinating section, surprising in the openness of discussion and lack of acrimony. One learns a lot in this section from page 295 to 302.

The following section is concerned with East and South Asia.

Tasuku Kimura and Hideo Takahashi. Pp. 305-320. "Cross Sectional Geometry of the Minatogawa Limb Bones"

The partial skeletons of four individuals and other isolated bones were discovered at the Minatogawa cave site on Okinawa Island. The materials present one of the largest assemblages of postcranial human skeletons reported from a Late Pleistocene site in East Asia and are therefore important for the discussion of fossil humans in this region."

"The present authors (Kimura and Takahashi 1984) have reported on the cross sectional geometry of the mid-length of the lower limb bones of the Minatogawa humans. In the present report, we measured the geometry in serial cross sections of the long bones from both the upper and lower limbs. The resultant cross sectional geometry indicates the robusticity of the long bones in relatively simple parameters."

"The absolute robusticities of Minatogawa long bones are small compared with recent or Jomon bones from Japan.

The limb bones of the Minatogawa humans are rather short in length. These characteristics coincide with other bone fragments discovered from Late Pleistocene sites in East Asia. The relative robusticities of the Minatogawa bones are in the range of recent bones when controlled by the lengths of the bones. Even considering the short length, the limb bones from Pleistocene sites in East Asia are not mechanically robust in contrast with those from the western part of the Eurasian Continent."

"East Asian fossil humans, including *Homo erectus* specimens, generally show limb bones without the outside robustness which is pronounced in those of all archaic and modern fossil humans from Pleistocene sites in West Eurasia. Not only are there differences between East Asian and West Eurasian limb bone specimens within each of these regions, there are also common features of general limb bone robusticity throughout the Middle and Late Pleistocene. Regional continuities in the robusticity of the limb bones are thus presumed to have been present on the Eurasian continent. The functional adaptations of the femur and tibia seem to have been the first to appear in the directional differences of robusticity against bending movements. This occurred in the context of similar sexual differences in the robusticity of the limb bones in both fossil and recent humans."

Nguyen Lang Cuong. Pp. 321-335. "A Reconsideration of the Chronology of Hominid Fossils in Vietnam"

"Many of the human fossil remains discussed here are separate teeth, with the exception of a unique fragment of frontal bones from Keo Leng. There is also the ancient skull from Water Shelter, which is almost intact but only semi-fossilized and latest in chronological order (early Hoabinh)."

"From cave excavations the typical genera and species of the *Pongo-Stegodon-Ailurapoda* complex have been found. It is safe to say the fossilized fauna of the Quarternary in North Vietnam were about the same as those of Guangxi Province in China."

"In Tham Khuyen Cave (Vietnam), there was no presence of species such as *Mastodon* sp., *Stegodon preorientalis*, *Equus yunnanensis*, and *Hyaena brevirostris licenti*, which are considered characteristic of the Lower Pleistocene or the beginning of the Middle Pleistocene in South China. On the other hand, apart from *Pongo*, *Ailuropoda* and *Stegodon*, we have discovered *Homo erectus* and *Gigantopithecus*, which are diagnostic fossils for the Middle Pleistocene. Compared with the faunas in Indonesia, the Tham Khuyen fauna has no such earlier elements as were found at Satir, Ci Saat, Trinil H. K., and Kedungbrubus. On the other hand, the Tham Khuyen fauna was earlier than the Ngandong fauna. In comparison with the fauna from Tongzi, Guizhou Province, which was classified by its excavators as Middle Pleistocene, the Tham Khuyen and Tham Hai Cave fauna are very similar, asserted to be from the early period of the Late Middle Pleistocene, in other words, from the last stage of the Mindel-Riss Interglacial Period, that is, about 250,000 BP."

"Judging from the similarities of many aspects of the Tham Om fauna, and that from Hang Hum I (the Tham Om

hominid tooth is even earlier), Maba, Dingcun, and Changyang, we assume the earliest sediments of Tham Om could be dated back to the early Upper Pleistocene or the late Middle Pleistocene, that is, about 125,000 BP."

"It is generally accepted by many scholars that the Keo Leng hominid is from the Upper Pleistocene is about 30,000 BP [sic - HF]"

[Note: given the rarity of Vietnamese fossils, it is important to say that some crucial parts of this paper are not found in the summary abstract. In his diagram Fig.18.4, he stipulates sites, dates and hominid categories on the "Eastern front". Using three categories, to wit, *Homo erectus*, early *Homo sapiens*, and *Homo sapiens*, he locates *Homo sapiens* as early as ca. 80,000 (Jiande), ca. 70,000 (Dali), ca. 55,000 (Salawusu), ca. 42,000 (Ordos), then three contemporaries at Lang Trang (Vietnam), the well-known Niah, and Wadjak. However, what he calls early *Homo sapiens* can be found ca. 300,000+ at Tongzi, 200,000+ at Tham Om, and two sites ca. 110,000 (Maba, Changyang). The first two, if confirmed, would effectively destroy the African origins hypothesis, or support the multiregional, however one might phrase it. Since his focus was on chronology, this heightens the importance of his diagram. HF]

[Another note: Jeffrey Schwartz does not appear in Cuong's bibliography, since his discoveries are more recent. We have solicited a short notice from Jeffrey about his newly announced "fossil hominoid from the Pleistocene of Vietnam". HF]

Arun Sonakia. Pp. 337-347. "Human Evolution in South Asia".

"Elimination of the biostratigraphically well-establish and spatially widely disseminated hominoid *Ramapithecus* from the hominid ancestral lineage on the basis of shared possession of derived characteristics (Kay 1981; Kay and Simons 1983; Martin 1985) gives rise to a controversy. Hitherto, *Ramapithecus* in the Siwalik Hills was believed to be the precursor of a much later fossil of *Homo erectus* located in the Narmada Valley in central India (Sonakia 19841, 1984b). But elimination of *Ramapithecus* from this sequence implies a much later migration of hominids to South Asia. This view is supported by the fact that there is an astonishing gap of evidence related to hominid evolution/phylogeny in the Siwalik beds. A gap of about five million years from Siwalik *Ramapithecus* to the much younger Narmada *Homo erectus* suggests that *Ramapithecus* could have migrated further east to eventually become *Pongo* (Orangutan) without leaving any successors to develop into *Homo erectus*."

"The group of fossil finds consisting of *Homo erectus* from central India, the anatomically modern *Homo sapiens* from Batadomba Lena cave in Sri Lanka (Kennedy and Deraniyagala 1989) and northeastern Afghanistan (Dupree 1972), and further evolved models from Mesolithic Sarai Nahar and Mahadaha in northern India (Dutta 1971; Sharma et al. 1974) suggest that collectively they could have been successors of these believed hominid immigrants."

"The presence of still primitive forms of *Homo erectus* (Wood and Chamberlain 1985) in distant Indonesia (Sangiran 800,000 BP) and China (Lantian 700,000 BP) cannot be explained on the basis of this later view of delayed immigration. These finds suggest that *Homo erectus* had travelled further into East and Southeast Asia after emanating from Africa, the believed epicenter of migration, long before reaching South Asia. In a way the Narmada fossil is younger than those at the Far East, the periphery of the ("out of Africa") migratory route. This contradicts the temporal continuity of the widely propagated unilateral migratory dispersal route ("out of Africa" model). This is contrary to the widely propagated unilateral migratory dispersal route."

"To ascertain the actual evolutionary course, the Siwalik strata together with contemporary Plio-Pleistocene formations of South Asia will have to be studied in much detail. Absence of evidence of links between *Ramapithecus* and *Homo erectus* will stand as evidence of absence of any link between the two and supports much later migration of *Homo erectus* to South Asia."

"Hence, it can be suggested that in South Asia *Homo erectus*, after attaining stability, underwent a multi-directional radiation, and this was followed by local independent evolution in pockets as suggested by the polycentric view of evolution or the the independent local evolution models."

[Note: I'm not sure I could explain his reasoning to anyone. His final conclusion — which does not seem to follow from his argument — is (p. 344): HF]

"For other centers of local evolution such as Southeast Asia and East Asia (Coon 1963; Aigner 1974, 1978), a continuity of evolution *in situ* from *H. erectus* to modern *H. sapiens* without any replacement has been demonstrated. Hence, the author is of the opinion that the South Asian group also documents a continuity from *H. erectus* onwards and is also one of the centers of polycentric evolution."

[Note: he presents two data. The Batadomba Lena site in Sri Lanka has a modern human skeleton and dates of 28,000-16,000 BP. It was reported in *Current Anthropology*, 1989, vol. 30, 394-399. And Louis Dupree reported a Neanderthal from Dara-i-Kur, Afghanistan. I am unable to find the dates for it. HF]

Hisao Baba and Fachroel Aziz. Pp. 349-361. "Human Tibial Fragment from Sambungmacan, Java"

"A hominid tibial fragment (Sm 2), recovered in 1977 from the short-cut canal of the Solo River in Sambungmacan, Java is described. The fragment, from the lower shaft of the right tibia, is inferred to be derived from the Kabuh-equivalent layer. The specimen is black in color and well-fossilized. The fragment's triangular cross section at midshaft is most like recent and Zhoukoudian *Homo erectus* tibiae and differs from other prehistoric tibiae compared. Comparing the diameters (32.0 and 21.2 mm) and flatness (66.3) of the midshaft, the Sambungmacan tibial fragment is closest to East Asian Mesolithic and Neolithic males and somewhat similar to Zhoukoudian *Homo erectus*. The Sambungmacan specimen



differs from Solo A and B, and is very different from European Neandertals and Upper Paleolithic *Homo sapiens*. However, the cortex of the Sambungmacan fragment is extremely thick and the medullary cavity is very narrow, resembling the limb bones of Zhoukoudian *Homo erectus*. Overall, the Sambungmacan tibial fragment exhibits surprisingly modern characteristics in spite of its great antiquity."

[Note: a piece of a limb bone, dated vaguely and geologically. HF]

Qiu Zhonglang. Pp. 363-372. "The Stone Industries of *Homo sapiens* from China"

"In China the stone industries of early *Homo sapiens* in the Middle Paleolithic are typologically similar to those of the Early Paleolithic. The stone industries of *Homo sapiens sapiens* in the Late Paleolithic can be divided into three categories: the traditional industries scattered all over the country; the blade industries found only in the Ordos area; and the microblade industries that mainly existed in the north of China. The latter two industries are believed to have developed locally in China."

It is no small matter how he defines his terms: voilà!

"The materials of the stone industries which will be discussed in this paper were collected from Middle and Late Paleolithic sites in China. Some of these industries are associated with the fossils of *Homo sapiens*, and the others, though not accompanied by *Homo sapiens* remains, are also believed by the author to have been produced by *Homo sapiens* because they share the same geological age."

"The term 'Middle Paleolithic industries' means the industries of early *Homo sapiens*, and 'Late Paleolithic industries' refers to the industries of *Homo sapiens sapiens*. As to their geological ages, the former date from the late Middle to early Upper Pleistocene, and the latter to the late Upper Pleistocene."

[Note: this is a size 6 shoe, and you take a size 6 shoe, so just squeeze your foot into the shoe — it must fit. HF]

Nevertheless there are some 46 sites enjoying these labels and scattered all over China. A tremendous amount of work has been done. If and when they get their dating squared away, these sites should have a terrific scientific impact, for or against the two contending theories of modern human origins.

Wu Xinzhi. Pp. 373-378. "The Origin and Dispersal of Anatomically Modern Humans in East and Southeast Asia"

"Common features found on the skulls of Pleistocene hunters in continental East Asia and other evidence favor the theory that *Homo sapiens* in this area developed from the indigenous *Homo erectus*, that is, that the anatomically modern humans of this region originated in an indigenous earlier human population. There were two branches of proto-Mongoloids during the Upper Paleolithic. The southern branch dispersed to

Japan and island Southeast Asia as well as to the remote Pacific islands and Australia. The northern branch migrated to the Americas via the Bering land bridge during the Late Pleistocene."

Johan Kamminga. Pp. 379-400. "New Interpretations of the Upper Cave, Zhoukoudian" [Note: Choukoutian to old timers - HF]

"The human burials from the Upper Cave have figured prominently in reconstructions of East Asian human evolution and prehistory. Since the 1930s these remains were thought to show regional continuity between the *Homo erectus* fossils of Zhoukoudian and the modern populations of China or more generally East Asia and the Americas. The results of a number of recent statistical studies of the "Old Man" cranium, as well as evidence that the burials may not be very ancient, allow for the possibility of a recent population replacement in North China, perhaps associated with the introduction of grain cultivation. The various statistical studies, which rely on Howell's samples of world populations, show that the shape of the "Old Man" cranium is not like that of East Asian and New World Mongoloids. Yet this cranium is fully modern in terms of shape and is even close to the average shape of modern human crania."

"Reliable dating of the Upper Cave burials is crucial in reconstructing population history in East Asia. Conventional C-14 dates on animal bones indicate a probable age of about 11,000 years. There is also other evidence suggesting a terminal Pleistocene or Early Holocene date. For instance, ornaments found with a human burial just under the cave's roof were similar to those of burials much lower down in the deposit. The dating issues is not yet settled, since a recent series of AMS radiocarbon dates has been interpreted to show that the human burials are older than 11,000 years. A difficulty in interpreting the published radiocarbon dates is that they were determined from animal bones rather than any of the human bones or ornaments. The deposit in the vicinity of the main series of burials has been disturbed by burrowing animals, and animal bones (and a sea-shell ornament) have moved downward a meter or more. New conventional radiocarbon dates were apparently determined by Chinese scholars from the human bones. These dates are still unpublished."

"Further study of the excavated material from the Upper Cave is necessary to resolve some of the more important issues. Perhaps highest on the agenda is compiling an inventory of the bones now held by institutions in Beijing and other cities in China. Much more can be done with the faunal assemblages and reconstruction of one of the female human crania should also pay dividends."

Günter Bräuer. Pp. 401-413. "The Origins of Modern Asians: By Regional Evolution or by Replacement?"

"From some recent publications one can get the impression that the problem of modern human origins can be reduced to rather simple alternative: Either modern humans evolved only in Africa and from there spread all over the world completely replacing all regional archaic humans, or else they

evolved in various parts of the Old World from regional archaic ancestors. The present paper shows that these simplified alternatives have arisen especially during the public debate between the few supporters of extreme positions. Moreover, this vigorous controversy distracts from the weaknesses of both models. On the one hand, there are some basic problems with regard to the complete replacement view, and on the other hand, the "Multi-regional Evolution" model is increasingly losing ground. However, a less extreme "Out of Africa" model suggested about ten years ago, which assumes a complex hybridization and replacement process, still appears to be in good agreement with current evidence from the western part of the Old World. In this paper the problem of whether one of the extreme positions or such a complex hybridization and replacement model offers a reasonable and realistic perspective for East Asia will be examined."

[Note: Günter Bräuer is a world class paleoanthropologist, a long ranger, and one whose views have not been adequately conveyed to ASLIPers (mostly by accident). His position here is one which many long rangers would prefer to adopt. For example, one of our founding members, an ethnologist, expressed his complete inability to believe in the replacement model because "everything he knew about anthropology" was opposed to it and in no uncertain terms. Professor Bräuer, who is a colleague of Ekkehard Wolff at the University of Hamburg, will satisfy our old ranger. Bräuer's conclusions about East Asia follow below: (pp. 407-409) - HF]

"In order to arrive at a more *realistic* model for the origin of modern East Asians, we should consider three further aspects:"

"1. Although there is an increasing number of fossils from East Asia (Wu Xinzhi and Wu Maolin 1985; Jacob 1981, personal communication; Sartono personal communication), there is still a gap in the fossil record between archaic specimens like Jinniushan, Dali, Maba, Ngawi or Ngandong and early modern humans like Longtanshan, Liujiang, Upper Cave, Niah, and Mungo. The Maba fossil, the most recent better preserved archaic *sapiens* specimen from China, dates to about 130,000 years BP (Wu this volume). Between Maba (Fig. 24.3) and the earliest modern remains is a gap of nearly 100,000 years. From my own personal observations, the remains from Xujiayao which might date in between are too fragmentary and morphologically quite heterogeneous to be of much help in clarifying what happened during these 100,000 years."

"In Australasia, the situation is quite similar. Human populations of the Ngandong-type were living in Indonesia around 80,000 to 100,000 (Bartstra et al. 1988). These archaic humans, considered by some specialists to represent late *Homo erectus* (Stringer 1984) lived only about 50,000 years before the earliest fully modern and rather gracile people known from Indonesia and Australia (Fig. 24.4). There is an unbridged gap between morphologically very different humans. The time period would thus be rather short for assuming indigenous evolution in this region (Bräuer 1989)."

"2. Another point is that a number of early modern specimens from China and Australasia exhibit basic similarities with early modern humans from Europe and even Africa, a fact which appears more difficult to understand by assuming different indigenous evolutionary lineages connected by gene flow over such a long time. Recently, Howells (1989:68) published the results of a PCA based on 50 modern and pre-modern samples, and more than 50 cranial measurements. The Upper Cave 101 cranium exhibits closest affinities to two early Holocene Elmenteita specimens from Kenya. Keilor from Australia was closest to Fingira from Malawi and next to Mladec 1 from Czechoslovakia, whereas all archaic specimens have much greater differences to all modern groups from all over the world. In another multivariate study, Habgood (1986) grouped the robust and gracile late Pleistocene/early Holocene crania from Australia separately and also included material from China, Indonesia, Africa and Europe. The results yielded surprising affinities: both the robust and gracile Australians were very close to each other and closest to a Taforalt specimen from Morocco and to Upper Cave 103 (and 102) as well as to an Upper Paleolithic cranium from Dolni Vestonice, Czechoslovakia. There are other results pointing in the same direction (Wang and Bräuer 1984; Kamminga and Wright 1988; Kamminga this volume; Van Vark and Dijkema 1988)."

"3. Finally, the Australian researchers Groves (1989) and Habgood (1989c, 1991) have questioned the value of nearly all of the assumed regional East Asian and Australasian features. Habgood (1989c) even found many of the Australasian features in the 10,000 years old Wadi Halfa from Nubia. The dubious status of many of these supposed regional features — most of them are retained primitive ones — was also shown by Stringer's (1991) assessment of their occurrence in late Pleistocene North African samples. To summarize this last point cautiously, it is quite evident that the number of useful regional features has decreased heavily. Therefore, the main pillar of the "Multi-regional Evolution" concept for this part of the world became rather weak."

"In view of these three basic problems, I find it difficult to assume that the occurrence of regional features in some specimens is sufficient proof for a truly indigenous evolution in the mainland of East Asia or in Australasia. On the other hand, it makes a complete replacement model unlikely as well."

[Note: for these reasons, Bräuer prefers a Hybridization and Replacement model. Since the word "mostly" seems to be lurking in the background, we might call it the "modified replacement" model. Two non-fossil issues would be (a) what the marriage rules were and (b) how close to being different species were the interacting (local and immigrant) populations. Might we not have the famous horse, ass, and mule problem? - HF]

Christy G. Turner II. pp. 415-438. "Microevolution of East Asian and European Populations: A Dental Perspective."

"The major findings of a 20 group multivariate analysis of 29 dental crown and root traits in several thousand



crania are: (1) North Asians and Europeans have evolved distinctly different dental patterns, with the former exhibiting more complexity, and latter more simplification. (2) Both patterns arose in Late Pleistocene times. The European pattern is at least 20,000 years old according to comparisons with Soviet Cro-Magnons, and the North Asian pattern is at least 12,000 years old based on comparisons with Asian-derived Paleo-Indians. (3) No pre-Neolithic dental clines have so far been found across Siberia. Instead, there seems to be a boundary at about Lake Baikal, where to the west occur Paleolithic Europeans like the 22,000 year old Mal'ta child and the 35,000 year-old Altai Mousterians, and to the east, the 8000 year-old Mongoloid Shulka male. (4) The north Asian pattern is called Sinodonty, and occurs in Japanese, Chinese, Mongols, Northeast Siberians, and all Native Americans. (5) Throughout Southeast Asia occurs another dental pattern I call Sundadonty. This pattern is also found in Ainu without Japanese admixture, Jomon people, and Polynesians. Similar frequencies of key Sundadont features seem to occur in Australian aboriginals. (6) In the 20 group matrix, both recent and early Southeast Asian Sundadonts have the lowest average Mean Measure of Divergence (MMD=0.098); North and South American Indians have the largest (0.290), slightly more than Africans (0.288). The mean world MMD in this matrix is 0.177, which is probably excessive due to over-representation of Sinodont groups. These divergence values suggest that all modern human dentitions could have more easily evolved from the Sundadont pattern than from the African, North Asian-American, or European patterns."

[Note: Christy Turner is well known to long rangers so we need not discuss his article, except that his last sentence seems new to me. Also how long have those 35,000 year old Altai Mousterians been known? - HF]

[Further note: I accept Christy Turner as our global dentist and explicitly accept his conclusion that Sundadonty is the least divergent dental pattern of human patterns since ca. 25,000 BP. I will have to disagree with this distinguished colleague on the interpretation of Sundadont's centrality. The center of a distribution, the point of least divergence, might be the origin point of that distribution. Sure, but it might not be either. This kind of reasoning is familiar to us from dialect studies. Thus the center of Romance dialecting might be between Nice and Trieste, while Sardinian dialects were seen as "extreme" or most divergent. Yet Sardinian is the oldest offshoot of Latin and it is close geographically to Rome, the known source of Romance. When Christy Turner argues that African teeth are extreme, he actually helps support the "out of Africa" hypothesis. - HF]

C. Loring Brace and David P. Tracer. Pp. 439-471. "Craniofacial Continuity and Change: A Comparison of Late Pleistocene and Recent Europe and Asia"

"The northeastern and northwestern edges of the Old World have many elements of similarity and some differences in terms of the circumstances that shaped human form in the Late Pleistocene and Holocene. Archaeological evidence

suggests that continuous occupation north of the 45th parallel had a considerably greater antiquity in the western (Strauss 1989) than in the eastern (Dolitsky 1985) extremes of human habitation. This is consistent with the differing extent to which the loss of the presumed ancestral amount of skin pigmentation has proceeded."

"Relative dental reduction in both areas has proceeded to almost exactly the same extent. Where tooth size is calculated in proportion to body size, the TS/BS index is 0.63 for Ainu and 0.64 for Nowwegians — the smallest figures recorded for modern *Homo sapiens* — as opposed to 0.85 for southern Australians — the largest for living humans (Brace et al. 1991). It is not yet possible to compare the two areas in terms of the amount of Late Pleistocene as opposed to post-Pleistocene contributions to that reduction."

"Both extremes saw the development of intensive exploitation of marine resources as the Pleistocene gave way to the Holocene (Bailey 1983; Ikawa-Smith 1986), and both areas were later entered by populations of farmers who were morphologically different from the *in situ* hunter/gatherers (Brace et al. 1989; Brace and Tracer n.d.). In both areas, there is evidence that the craniofacial features now found at the extreme peripheries — England and Norway in the west, Hokkaido in the east — once had a wider distribution in adjacent areas."

"The record of terminal and post-Pleistocene expansion from the two areas is very different. Minor offshoots from the northwest settled the Faroe Islands and Iceland. The terminal Pleistocene inhabitants of the northeast are the most likely source for the expansion across the Bering Strait into the western hemisphere on the one hand, and south via the Ryukyus, Taiwan and the Philippines and out into remote Oceania on the other hand. Jomon and Ainu craniofacial form links more closely with Amerind, Eskimo, Micronesian and Polynesian than any of these link with the Asian mainland."

"The mainland group that is most morphologically distant from the others, and the last to link with any cluster, is the Mongol sample (Li et al. 1991). The word "Mongoloid" is the poorest possible term that could be used to characterize any of the peoples of Asia except those from Mongolia."

[Note: C. Loring Brace is another world class physical anthropologist, famous for textbooks and statements on race. We do not pursue his long article because its relevance is not great but his last sentence in the penultimate paragraph would be contradicted by Turner's dental studies and Cavalli-Sforza's biogenetics. The Wallace team's mtDNA studies support Brace in the Pacific. - HF]

[A further note is necessary, since his abstract did not say all that he was doing. At the end of his conclusions, he discusses the main point of his paper which, after all, is relevant. Try to remember that he is basically talking about *faces*. - HF]

Thusly (page 463):

"The Cro-Magnon configuration clearly has a continuity of at least 30,000 years in western Europe. Since

cladistic treatment can identify Neandertal and modern western European forms as earlier and later versions of that same configuration (Brace 1991:145-148, n.d.), this provides another piece of evidence, comparable to that of Qafzeh and the East Asian material discussed above, that the basic non-adaptive aspects of craniofacial form tend to persist over many tens of thousands of years. As with the absence of any archeological support for major population movements out of Africa at the end of the Middle Pleistocene, these data are flatly incompatible with the view that modern Europeans on the one hand and modern Asians on the other were derived by transformation from migrants out of Africa within the last 100,000 years (Stringer 1990:101)."

The discussion following the third day of papers was chaired by Geoffrey G. Pope and Christy G. Turner II. It must have been rather exciting. From two and a quarter pages of reportage, it is evident that three things can be said of it. First, read it yourself. Second, "Despite Bräuer's disapproval of extreme antipodal positions, it is safe to say that none of the participants altered their opinions regarding the origin and dispersal of humans in Asia." And third, that scholars who work on the Western Front do not appreciate the amount and quality of work being done on the Eastern Front, especially by Chinese and Japanese scholars publishing in their native languages.

For the remainder of the papers only the abstract is given, just because of the limitations of space. Cavalli-Sforza's paper is not summarized because his huge book will be reviewed in our next issue. But one conclusion is quite clear by now. Fancy statistics or not, cladograms, MMDs, and all kinds of analyses by smart people seem nevertheless to produce *different results*, depending on whether they use teeth, biogenetics, or craniometry. And my frank guess is that the problem lies with craniometry.

Yukio Dodo, Hajime Ishida, and Naruya Saitou. Pp. 479-492. "Population History of Japan: A Cranial Nonmetric Approach"

"The incidence data of 22 cranial nonmetric traits were analyzed in 16 cranial series, of which ten are from Japan and six from overseas. It was demonstrated that the incidence pattern of cranial nonmetric traits reflected the Japanese genetic constitution of historic times when no significant gene flow from abroad was evident in Japan. Concerning the population history of Japan, distance analyses based on trait incidences indicated that (1) the Jomon and Ainu are closely related to each other; (2) there existed population discontinuity between the Jomon and the Yayoi; and (3) the genetic constitution of continental immigrants such as the Yayoi of northern Kyushu had predominated over that of the natives in various parts of western Japan by around the middle of the Yayoi period, and this resulted in the appearance of the direct ancestral population of the modern Japanese."

Nancy S. Ossenberg. Pp. 493-530. "Native People of the American Northwest: Population History from the Perspective of Skull Morphology"

"It has been proposed that the New World was peopled by three migrations from Northeast Asia during the late Pleistocene. The earliest (Macro-Indian) gave rise to South, Central and North Amerinds except those derived from the second migration. The second migration was ancestral to Indians of the Alaskan interior and North Pacific Coast (mainly Na-Dene). The most recent migration brought the Aleut and Eskimo, who share a single language phylum and, presumably, a biological kinship distinguishing them from all other Amerinds (Greenberg, Turner and Zegura 1986)."

"This paper examines the evidence for Aleut, Eskimo and Indian relationships in a broad context of comparisons provided by frequencies of 25 nonmetric cranial traits scored by one observer in 55 sample (N=3487). The Mean Measure of Divergence (MMD) statistic was used to estimate biological distance between populations. Further analysis of MMDs included testing for rank correlation between samples using the Spearman's *r*, and cluster analysis to construct dendrograms."

"Dendrograms including all samples had four clusters joined in this order:

1. all 39 Amerind samples plus 3 Chukotkan
2. 3 Tungus, 2 Japanese, plus Yukaghir
4. Ainu and Jomon
5. 2 Black (linked together) and 3 Indo-European (linked)

"Thus, at least according to these few samples, cranial nonmetric traits agree with dental morphology that the primary split among the major populations worldwide separates Asian and Asian-derived people on the one hand, from Africans and Indo-Europeans on the other." [Note: terminological sloppiness - HF]

"Mean MMDs for 8 Amerind aggregates ranked Northeast Asians as follows, from closest to most distant. Chukotkans, recent Japanese, Tungus, Ainu, Jomon. These particular Americans appear to have derived from an Arctic Mongoloid stock rather than from the Classic Mongoloid stock ancestral to Tungus and Japanese. In keeping with the archeological record (Chard 1974) and dental morphology (Turner 1985), Neolithic Japan apparently exerted little influence on the New World."

"When only the 7 Aleut and 11 Eskimo samples were considered, the results of cluster analysis and the significant rank correlation of MMDs with linguistic and geographic distances could easily be adduced in support of the monophyletic theory of Eskimo-Aleut origins; as well as the reconstruction that southwestern Alaska was the ancestral homeland of the stock, from which it expanded westward into the Aleutians, north along the Bering Sea coast, and then east along the Arctic coast (Laughlin et al. 1979). With the addition of Indian samples, however, the picture became much more complex."

"The 39 Amerind samples, representing descendants of the two most recent migrations hypothesized, did indeed distribute themselves consistently in two major sub-clusters in the dendrograms. But in disagreement with the prevailing view,

and totally at odds with their linguistic affiliation, Aleut joined Na-Dene and Northern Plains Indians rather than Eskimo. Additional analyses, including dendrograms constructed from MMDs based on a subset of the 15 traits most powerful for Eskimo-Indian discrimination, consistently placed Aleuts with these particular Indians rather than with the Eskimo. These relationships hark back to earlier views put forward independently by Hrdlicka (1945) and Neumann (1952), and are supported by a recent multivariate analysis based on Hrdlicka's measurements (Brennan and Howells 1976). Review of serological, somatological, dental and other cranial studies produced little that could convincingly dispute these findings."

"An attempt to interpret the findings led to this reconstruction. The two Amerind subclusters seen in the dendrograms represent two migrations. The earlier, "Paleoarctic", at about 9000 BC gave rise to Aleut, Na-Dene and other northwest Amerinds. A later one, "Neoarctic", of a different genetic stock, gave rise to Eskimo. A north-south gradient of affinities reflected in the ranked MMDs (Chukotkans, Inupiaq Eskimo, Yupik Eskimo, Athapaskans, Aleut, Haida/Tlingit, Northern Plains) was the product of demic diffusion associated with the Norton tradition at about 500 BC, but most particularly with the Thule expansion at about AD 1000. The impact of the Neoarctic immigrants was strongest close to the Bering Straits at the point of entry and eastward along the sparsely inhabited Arctic coast (Inupiaq), decreased toward densely populated southwestern Alaska (Yupik), and minimal in Indians of the Alaskan interior (Athapaskans). The southern end of the cline (from Na-Dene through Plains), perhaps reflects gene flow between Paleoarctic peoples and descendants of the earliest (Macro-Indian) immigration. Aleut, enclaved in their archipelago, remained isolated from Neoarctic genetic influence. Thus, at the time of Russian contact, they represented a relict Paleoarctic population, much as the Ainu at the northeast margin of the Japanese archipelago represented a relict descendant of Jomon."

"Vexingly elusive with this scenario is a good explanation for the linguistic affinity of Aleut to Eskimo. The explanation may well remain hidden in crucial archeological sites now submerged, or in Eskaleut languages now extinct. But if the answer is found, it could provide insights useful for drawing microevolutionary parallels and tracing other paths in the dispersal of the great Mongoloid race."

[Note: for reprint seekers, Ossenberrg is/was at Queen's University, Dept. of Anatomy, Kingston, Ontario, Canada. This is a 38 page article, full of data and diagrams, well worth getting a copy of or even of buying the book for. The elusive factor is her inability to think about linguistic groups historically and her tendency to lump discrete entities together. Early Eskimoan or perhaps only proto-Aleut encountered resident Na-Dene in southwestern Alaska, got plenty of genes through intermarriage, and then spread westward along the Aleutian chain. That's hard? -HF]

Michael Pietrusewsky, Li Yongyi, Shao Xiangqing, and Nguyen Quang Quyen. Pp. 531-558. "Modern and Near

Modern Populations of Asia and the Pacific: A Multivariate Craniometric Analysis"

"Discriminant function analysis and Mahalanobis' Generalized Distance measurements are applied to 34 cranial measurements recorded in 2275 modern and prehistoric human crania from Asia and the Pacific for assessing the historical-biological relationships of these populations. The samples include 8 East Asian, six mainland and eight island Southeast Asian, six Polynesian, three Micronesian, eight Melanesian, and five Australian and Tasmanian groups. The results of two separate analyses using 22 and 44 male samples, respectively, are reported. A relatively homogeneous Asian populations complex with regional differentiation between northern and southern members is manifest. Bronze-age Chinese align with modern Chinese and Neolithic Thai resemble modern Southeast Asians. Variations in facial and frontal breadth, facial height, malar length and minimum cranial breadth are primarily responsible for group differentiation in the first analysis. Broader comparisons indicate a marked separation between Asian and Australo-Melanesian groups. Polynesians and Micronesians are tangential members of the Asian complex. In addition to the variables recognized in the first analysis as important discriminators, palate length is primarily responsible for the group separation in Analysis 2. The modern peoples of East and Southeast Asia and much of island Oceania share a common origin distant from and unrelated to the indigenous inhabitants of Australia and Melanesia."

[Note: hundreds of crania were examined and analyses generally back up their statements. Usually. However, after they have stressed the chasm between the East/Southeast Asians and the Australo-Melanesians, their dendrogram (Fig.30.5, page 552) lumps Neolithic Thailand and Tasmania as very close, as close as Hangzhou and Shanghai. An error in printing? Or one in thinking? - HF]

Sandra Bowdler. Pp. 559-589. "*Homo sapiens* in Southeast Asia and the Antipodes: Archeological Versus Biological Interpretations"

"Archeological evidence for the occupation of Australia suggests people arrived there ca. 40,000 or possibly 50,000-60,000 years ago (Allen 1989; Roberts et al. 1990)."

"The earliest human remains are those of two individuals, one female and one male, from Lake Mungo in western New South Wales, dated to ca. 25,000 BP, and ca.30,000-28,000 BP, respectively. Both are considered to have extremely gracile skulls, and to represent fully modern humans, *Homo sapiens sapiens*. Another large collection of human skeletal remains comes from Kow Swamp on the Murray River. These individuals are considered to exhibit an extremely robust skull morphology and, while also considered to represent *Homo sapiens sapiens*, are nevertheless also thought to show archaic characteristics (e.g., Thorne 1977). The Kow Swamp burials are however all dated to between ca. 13,000 and 9000 BP. Other individual skulls are known which are considered to fall into either the "gracile" grouping (e.g., Keilor, ca. 12,000 BP?) or the "robust" grouping (e.g., Cossack, ca. 6000 BP?)"

"One interpretation of this evidence is generally known as the 'regional continuity' model (see, e.g., Wolpoff et al. 1984, Wolpoff 1989). This model sees an evolutionary line leading from *Homo erectus* at Zhoukoudian Upper Cave through Upper Pleistocene sites such as Maba, Liujiang and Zhoukoudian Upper Cave to the modern Mongoloid form of *Homo sapiens sapiens* on the one hand, and on the other, a line of descent from the Javanese *Homo erectus* of Trinil and Sangiran through the presumed later *H. erectus* known as *H. e. soloensis* to robust Australoids. The Lake Mungo-type gracile group are thought to represent an incursion of Mongoloids, who interbred with a pre-existing Australoid population, represented by Kow Swamp robust types, leading to modern Aboriginal populations in Australia."

"While this model is apparently attractive to biologists, who take into account primarily morphological features, it is extremely difficult to reconcile with the other evidence. Firstly, the date for the Mungo burials and the dates for the Kow Swamp burials are simply the wrong way around to support this interpretation. The model demands a considerably older dating for robust examples than any which exist. It also raises the question of why the robust morphology should persist so long. It is the case however that competing biologically-based interpretations have been offered by Brown (1989) and Pardoe (1988). The regional continuity model is in conflict with the "replacement" model which derives from DNA studies (e.g., Stoneking and Cann 1989)."

"A consideration of the archeological evidence which concentrates on cultural rather than biological aspects does not support the Wolpoff-Thorne model. This evidence must be considered on a regional scale to include not only Australia but also Southeast Asia and, as far as possible, China. In Southeast Asia, as in Australia, there is no clearly dated cultural evidence older than ca. 50,000 BP. There is no evidence that the Javanese *H. erectus* populations were culture-bearing creatures, let alone any evidence for cultural continuity between those and most recent populations. There are on the other hand sufficient cultural similarities between early Australian and Southeast Asian stone artifact assemblages to allow us to suggest some relationship between them. With respect to China on the other hand, the Zhoukoudian *H. erectus* group do seem to have been a culture-bearing community, and it can even be argued that there are suggestions of cultural continuity from the earliest Chinese industries through to the Upper Pleistocene, and that this may also embrace the early Southeast Asian and Australian industries.

"It is suggested therefore that the model which is best supported by the archeological evidence is as follows. In Southeast Asia, early *Homo erectus* populations represented a peripheral culture-less population which died out sometime during the Middle or possibly early Upper Pleistocene. Australia, and also Southeast Asia, were colonized during the later Upper Pleistocene by *Homo sapiens sapiens* populations with a Chinese origin, possibly with a cultural heritage stretching back to Zhoukoudian."

[Note: confirmatory archeological dates for early

settlements near Australia, presumably reached after the primary settlement of Australia, are given. In south Tasmania, 30,850 to 29,000 BP are recorded. In Melanesian islands, necessarily requiring a sea trip from New Guinea, dates from 18,560 to 33,300  $\pm$  950 BP are found on New Ireland; 20,000  $\pm$  to 29,000  $\pm$  BP are found on Buka; and on Margaret Mead's famous Manus, a date of 12,500 BP in top layers at 1.7 meters with "a further two meters of cultural deposit below this". Manus is 200 km out to sea! Early evidence of the Pacific Islanders as seafarers! - HF]

Shoji Harada. Pp. 591-598. "Molecular Basis of Alcohol Sensitivity and Its Application to Anthropology"

"During the last decade studies on alcohol metabolism have revealed that individual and racial differences in alcoholic intoxication are attributable more to acetaldehyde than to ethanol itself and that there are genetic differences in the enzymes involved in alcohol metabolism."

"In this paper, enzymological aspects of acetaldehyde metabolism are reviewed with particular emphasis on the genetically determined aldehyde dehydrogenase isozyme variation and its functional implications for alcohol sensitivity. Also, the incidences of aldehyde dehydrogenase variants in different ethnic populations are compared in order to obtain basic data concerning Mongoloid dispersals."

"The ALDH2\*2 gene is found only among Mongoloid groups but not in Caucasian and Negroid populations. The frequency of the mutant gene is highest in China and Japan. It is very rare in Papua New Guinea, Australian Aborigine and American Indian populations. The ALDH2\*2 gene may have appeared by point mutation of the normal allele in Neo-Mongoloids and might have dispersed to the neighboring populations by migration."

[Note: the symptoms are "facial flushing, tachycardia, headache, and peripheral vasodilation after drinking small quantities of alcoholic beverages." Because of the punishing quality of drinking alcohol for some, Harada suggests this as a reason for a lower rates of alcoholism among Chinese and Japanese, but about 75% of them lack this (terrible) gene. "Neo-Mongoloids" apparently = northern Mongoloids. Koreans have 15% of it too. - HF]

Katsushi Tokunaga and Takeo Juji. Pp. 599-611. "The Migration and Dispersal of East Asian Populations as Viewed from HLA Genes and Haplotypes"

"The human leucocyte antigen (HLA) or major histocompatibility complex (MHC) system includes several polymorphic marker genes which are tightly linked on the short arm of chromosome 6. A remarkable feature of the system is that each locus shows a high degree of polymorphism and particular sets of MHC alleles (haplotypes) occur much more frequently than expected from a random combination. Also there is molecular evidence in terms of gene organization that the MHC haplotypes serve as reliable and stable markers. It is of much interest that each characteristic haplotype shows a limited regional distribution and accordingly becomes an

excellent genetic marker for various human populations.”

“The distribution of MHC haplotypes in East Asian populations has been investigated in extensive family materials. Thus far 460 families collected from various districts of Japan, 100 families of Han Chinese from the Beijing area and 30 Korean families from Seoul have been analyzed for HLA-A, -B, -C, -DR, DQ antigens and for HLA-linked complement components C4A, C4B and factor B (BF). Population data that have been reported for other regional populations were also taken into consideration.”

“Significant differences in the frequencies of characteristic MHC haplotypes were found among different regional populations. The distribution of several common haplotypes suggests multiple migration and dispersal routes of such characteristic MHC haplotypes may contribute to our understanding of the evolution and formation of modern humans.”

[Note: they found that Japanese and Koreans were closest but Han Chinese not too far removed. They also present an “affinity network” which more aptly might be called a spidergram which shows major groups like Papuans, Africans, etc. well separated from each other but also shows Eskimos and Mongols and Nepali not very close to Korean and Japanese. This basically contradicts the Gamma Globulin evidence and reinforces Cavalli-Sforza’s opinion (during the discussion) that HLA is interesting but may be subject to considerable natural selection. - HF]

## RE-THINKING NATIVE AMERICAN PHYLOGENY: GENES VS LANGUAGES

The bold venture made less than a decade ago to propose three distinct linguistic phyla for the Americas, to link them with three distinct biogenetic clusters of native Americans, and to propose that they came to the New World in three distinct waves of immigration, beginning with the Amerind first settlement around 12,000 BP more or less, has been the dominant paradigm for long rangers ever since. The language taxonomy was proposed by Greenberg, the biogenetic (dental in this case) was mostly Christy Turner’s work, and the dates were furnished by Zegura’s archeology.

From the outset, as everyone knows so well, the linguistic classification was under heavy attack. It has survived quite well, however, because the attack was mostly ideological and border-line irrational (in some cases). However, the dates of first settlement were doubted initially in *Mother Tongue* (by Fleming 1987) and, as new archeological sites and biogenetic dates were reported, came to be believed in less and less. Greenberg has said (personal communication) that the archeological dates were a convenience and that the Amerind data really did look older than 12,000 BP. Finally, the biogenetic correlates with the linguistic phyla have threatened to come unglued because workers with mtDNA found *four* “lineages” rather than three. Some other biogenetic work did not agree, however, and the stage was set for a really thorough examination of the largest possible selection of the genes of native Americans, viz., Eskimos, Aleuts, Na-Dene, and all the rest (who are usually called Amerind in the narrower sense, or without Na-Dene).

Two such thorough examinations have occurred. As luck would have it, they do not agree with each other. The one, part of Cavalli-Sforza’s giant book (to be reviewed herein), primarily agrees with the tripartite scheme of Turner’s dental research. It should also be said again that Christy Turner has always put all the native Americans in his *Sinodont* taxon, making them northern Mongoloids in other people’s terms and their traditional designation in most text books of anthropology.

The other will be reported below by D. Andrew Merriwether, who has written this small report with long rangers in mind. Like a linguist trying to explain to a terrified archeologist how /č/ can come from /\*k/, he will try to explain to frightened linguists how one does all that confusing calculation of genes and haplotypes *und so weiter*. Then he will present his conclusions about the phylogeny of native Americans. He is supported by another paper, which we will copy, and by a guest editorial by Rebecca Cann, which we will present just after that.

Andy Merriwether is currently at the University of Pittsburgh, Department of Human Genetics, Graduate School of Public Health, A300 Crabtree Hall, 130 De Soto Street, Pittsburgh, PA 15261, tel. 412-624-3232 (lab) or 412-859-3185 (home). Fax no. is 412-624-3020. His internet address is

andym@vms.cis.pitt.edu, and his America Online address is ANDYM. In addition to your wanting to contact him about his research, he is just now a "postdoc" who is entering the job market. If your university wants to hire a brand new state-of-the-art fellow in this hot hot research, then get in touch with Andy. I am plugging him shamelessly because I admire this long ranger's work very much.

For linguists not wanting to read biogenetics and those salivating at the possible refutation of the Greenberg hypothesis, our advice is to relax. What this phylogeny is about is *carnal*, matters of the flesh but not of the spirit (where language lies). There surely will be revisions of the dominant paradigm referred to above, but this all should lead us to new understanding of the complicated history of man in the New World. Closer to the truth. We'll have to wed Cavalli-Sforza's genetics to those of the mtDNA folks. And of course somebody may turn out to be wrong!

## mtDNA AND THE PEOPLING OF THE NEW WORLD

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Several papers and abstracts have come out (or are in press) by D. Andrew Merriwether, Robert E. Ferrell (both at the University of Pittsburgh), and co-authors describing mitochondrial DNA (mtDNA) variation in a wide range of Native American populations. Since the analysis of this data has implications for the timing, route, frequency, and antiquity of migrations into the New World, this research may be of interest to the linguistic anthropology community. Two papers and a dissertation:

Merriwether, D. A., Rothhammer, F. and Ferrell, R. E. (1994). "Genetic Variation in the New World: Ancient Teeth, Bone, and Tissue as Sources of DNA." *Experientia* 50.592-601.

Merriwether, D. A., Rothhammer, F. and Ferrell, R. E. (submitted). "Distribution of the Four Founding Lineage Haplotypes in Native Americans Suggests a Single Wave of Migration for the New World." *Human Molecular Genetics*.

Merriwether, D. A. (1993). *Mitochondrial DNA Variation in South American Indians*. Ph.D. Dissertation, University of Pittsburgh.

provide new data to compliment and contrast the work from the Wallace lab at Emory (Schurr et al., 1990; Ballinger et al. 1992; Torroni et al., 1992a, 1992b, 1993a, 1993b, 1993c, 1994a, 1994b) and Ryk Ward and collaborators (Ward et al., 1991, 1993, 1994; Shields et al., 1992, 1993). Several of these were discussed in issue 21 (pp. 51-55) of *Mother Tongue* and earlier issues. In any event, here follows a summary of the research from Merriwether and Ferrell:

### Mitochondrial DNA

It is necessary to define some molecular biology terminology to describe the research. Mitochondrial DNA (mtDNA) is DNA found in an extracellular organelle called the mitochondrion. Each cell has as many as 750 mitochondria, each containing several copies of it's own mtDNA. This DNA is separate and different than the DNA found in the chromosomes in the nucleus of each cell. Each cell has one nucleus containing two sets of 23 chromosomes, one set donated by the mother, and the other set from the father. During meiosis, recombination occurs and the maternal and paternal copies of each chromosome recombine and exchange segments of DNA creating two hybrid chromosomes containing both maternal and paternal DNA. In



mitochondrial DNA, this does not happen. Only females transmit their mtDNA to the next generation. Male mitochondrial DNA is not transmitted to the offspring, thus we say that mtDNA is maternally inherited. It is not known precisely why this is so, but we do know that there are over 100,000 copies of mtDNA in the egg and only around 4 copies in the sperm. Presumably, there is some kind of exclusion mechanism to prevent the sperm mtDNA from entering the egg, or that destroys invading mtDNA from the sperm. The end result of this is that mtDNA is transmitted unaltered (except by random mutational events) from mother to offspring, generation after generation. MtDNA variation data therefore provides a maternal genealogy of the human race. MtDNA has a mutation rate 6-10 times higher than that of nuclear DNA, so we can expect to see 6-10 times more mutations between two pieces of mtDNA than we would between two pieces of nuclear DNA of the same size that have been separated for the same amount of time. This means that we can use mtDNA to examine the differences between populations and individuals that have only recently begun to diverge from one another. To identify this variation in the mtDNA, we use one of two methods: the first is to use restriction endonucleases (restriction enzymes) to cut the DNA and observe the patterns of the cuts; the second is to directly sequence a portion of the mtDNA (usually a region called the control region or D-loop, which contains nearly 30% of all the variation in the mtDNA).

### RFLP Variation

Restriction enzymes are used to generate what are called Restriction Fragment Length Polymorphisms (RFLPs). Restriction enzymes only cut at specific recognition sites containing specific sequences of nucleotides (bases). For example, the restriction endonuclease Alu I cuts DNA every time it locates the sequence AGCT (between the 'G' and the 'C'). If you digest a 24 base piece of DNA consisting of the sequence of

CAGGTATTAGCTAATACCGACTTA

with Alu I, you would end up with a 10 base piece (CAGGTATTAG) and a 14 base piece (CTAATACCGACTTA).

To identify these, you run them out on an agarose gel which separates pieces of DNA by size. DNA has a negative charge, so when you run a current across the agarose gel, the negatively charged DNA moves towards the positive electrode. The gel matrix separates DNA bands of different sizes because small pieces move more easily through the gel than do large pieces. By running a size standard next to the pieces of DNA you are examining in a gel, you can accurately size the pieces of DNA you observe. The DNA is made visible by staining it with Ethidium Bromide. Ethidium Bromide fluoresces under ultraviolet light, making the bands of DNA in the gel light up when exposed to UV light. There are hundreds of known restriction enzymes, each recognizing a different series of bases. If you cut a piece of DNA with many different restriction enzymes, you can construct a pattern of cuts and lack of cuts for

each individual. We call this patterns of cuts and lack of cuts a "haplotype". The more cuts (or lack of cuts) two individuals share, the more closely related they are presumed to be. The sharing of restriction site cuts (or lack of cuts) is presumed to be due to shared common ancestry (in other words the cut was presumed to have occurred in some ancestor common to both individuals). Restriction enzymes let us infer the sequences of DNA in individuals (only at cut sites). Alternatively, we can directly sequence a portion of DNA.

### Sequence Variation

When we compare the sequences of multiple individuals, we look for the differences between these sequences. Most base positions will be identical between any two humans you examine, even in the highly variable mtDNA, but there are differences nonetheless if you look at long enough pieces of mtDNA from two maternally unrelated individuals. If we compare the sequence (1) shown above with three other sequences:

		00000000011111111122222
position		123456789012345678901234
Sequence #	1	CAGGTATTAGCTAATACCGACTTA
Sequence #	2	CAGGTATTACCTAATACCGACTTA
Sequence #	3	CAGGTATTAGCTAATACGGACTTA
Sequence #	4	CTGGTATTACCTAATACCGACTTA

we can see that sequences 1 and 3 both would have been cut by Alu I, but sequences 2 and 4 would not have been cut by Alu I. We see other differences as well. Sequence 3 differs from sequence 1 at position 18 (the C becomes a G), and sequence 2 differs from sequence 4 at position 2 (the A becomes a T). It is possible to use algorithms to construct phylogenetic trees from either this sequence data or the RFLP data. The two most common methods are genetic distance based measures (UPGMA, Neighbor-joining, Fitch-Margoliash, etc...) and Parsimony. The genetic distance measures all start by counting the number of differences between all possible pairs of sequences or RFLP haplotypes. A genetic distance matrix is created, and the most closely related individuals (those with the smallest number of differences between them) are clustered together. This process is repeated until all sequences or haplotypes have been clustered together into a single phylogenetic tree. Parsimony tries to connect the sequences or haplotypes together by making the smallest possible number of changes. From the example above, you can connect sequence 1 and sequence 3 together by making a single change (changing position 18 from C to G). Obviously, the more sequences you have, the more possible ways there are to connect the sequences together, and the more complicated it becomes. In fact, a point is soon reached where there are a large number of equally short (equally parsimonious) trees. Then it is possible to construct a consensus tree from these equally parsimonious trees which identifies the relationships that are supported on many different trees.



## Four Mitochondrial Lineages for New World Founders

Schurr et al. (1990) showed that all Native American mtDNA variation fell into one of four major clusters. Each of these clusters was originally defined by a single RFLP or by a 9-base-pair (bp) deletion. These four clusters were considered to be descendants of the "four founding lineages" which peopled the New World. They were labeled A, B, C, and D. Lineage A was defined by a Hae III restriction enzyme cut at position 663, lineage B by a 9-bp deletion between the tRNA for Lysine and the gene for Cytochrome Oxidase II, lineage C by the loss of a Hinc II restriction site at position 13259, and lineage D by the loss of an Alu I restriction site at position 5176. The Hae III 663 site gain and the 9-bp deletion are largely Asian-specific, while the Hinc II and Alu I site losses were only Asian-specific when accompanied by two other restriction site gains (Alu 10397 and Dde I 10394). Thus, all Native Americans surveyed to date seem to fall into one of these four founding lineage clusters. Those few that do not all occur in populations with known amounts of Caucasian admixture. Later the Ward lab, Wallace lab, and Satoshi Horai's group in Japan were able to show the mtDNA D-loop sequence data generated the identical four clusters that were defined by RFLP variation.

Shortly after the original paper by Schurr et al. (1990), of which Merriwether was a coauthor, Merriwether joined the lab of Robert E. Ferrell at the University of Pittsburgh and began typing over 1800 Native Americans for the RFLPs that define the four founding lineages, as well as sequencing a portion of the D-Loop from over 250 native Americans. The populations surveyed by Merriwether and Ferrell included the following:

Aymara from 11 altiplano and 1 coastal village (from northern Chile); Atacameno (north-central Chile); Pehuenche from two villages (central Chile); Huilliche (coastal Chile); Yaghan (southern Chile); Quechua (Southern Peru); Yanomamo (Brazil); Muskoke (Oklahoma); Mohawk (eastern Canada); Dogrib (northwestern Canada); Yupik Eskimo from 12 villages (southwest Alaska); St. Lawrence Island Eskimo from two villages (Alaska); Pribilof Island Aleuts (St. Paul) and Eskimos from two villages. If one follows the more simplistic Greenberg classification of Amerind, NaDene, and Eskaleut, all of our samples were Amerind except for the Dogrib (NaDene) and the Eskimos and Aleuts (Eskaleut).

The data are graphed by population in Figure 1, showing the frequencies of the four founding lineage types (and all others combined into "other") as a series of 5 stacked histograms. Populations are arrayed in geographic order from South to North. The population size is indicated in the [brackets]. In our RFLP studies, we found lineages A and C in the Dogrib; A, C, and D in the Aleuts; A, B, and C in the Yanomamo; lineage C in the Yaghan (but we were not able to type most of them due to the poor quality of the DNA). Every other population we surveyed had all four of the founding lineage types present. Even the Eskimo had all four, although not in the same population (Old Harbor Eskimos had A, B, and

D; Ouzinkie Eskimos had A, C, and D; Southwest Alaskan Yupik Eskimos had A, C, and D). In addition, we surveyed several ancient populations by extracting DNA from bones, teeth, and mummified tissue. These were the Copan Maya (Honduras), the Fort Ancient Culture (Ohio and West Virginia), and the Chinchorro and Alto Ramirez cultures (Azapa Valley, northern Chile). The poor quality of the DNA has limited our results, but of special interest was the complete absence of the 9-bp deletion (defining lineage B) in all these populations, and especially in the Azapa Valley cultures. The nearby Aymara exhibited the deletion at 55-100%, the Quechua at nearly 40%, and the Atacameno at over 70%, whereas none of the two dozen Chinchorro and Alto Ramirez DNAs exhibited the deletion. Lineages C and D were observed in the Copan Maya, while all four lineages were observed in the contemporary (Yucatec) Maya from Mexico (with lineages A and B being the most common).

It is apparent from our data that one can find all four founding lineage types in each of Greenberg's three major groups (Amerind, NaDene, and Eskaleut) and therefore in the three putative waves of migration that have been said to correspond to them. Torroni and co-workers attribute this to admixture between Amerinds (which contain all four founding lineage types) and the other two groups. They suggest that the NaDene migration consisted solely of lineage A (and possible D), and the Eskaleuts of lineages A and D, whereas the presence of other lineages in these populations was due to admixture with Amerinds. Thus, the primary (Amerind) wave of migration contained all four founding lineages, and the two successive waves (NaDene and Eskaleut) contained only lineages A and D. Merriwether and Ferrell point out that virtually every NaDene and Eskaleut New World population surveyed to date has more than just these two lineages. It is therefore difficult to accept the argument that there is a difference in the origins of the three putative waves of migration, since all three putative waves seem to possess the same four founding lineage types (or subsets thereof). If there were separate waves of migration, possibly 7,000-25,000 years apart (5,000 ybp, 12,000 ybp, 30,000 ybp are common dates assigned to the three waves), then we would not expect these migrations to exhibit the identical four founding lineage types exclusively. Even if they came from the same putative Asian-Siberian founding population, the likelihood of drawing these same lineages three separate times from the pool of available lineages seems unlikely (unless these were the only lineages present in the parent population as well, and were maintained throughout that entire period of time). Both the Wallace lab and the Ward lab have provided divergence time and coalescence time data suggesting different divergence and/or coalescence times for the three putative waves, and a more recent divergence time for lineage B versus lineages A, C, and D. Merriwether et al. (1994, submitted) suggest that the difference in divergence times for lineage B versus lineages A, C, and D is due to the fact that multiple variants of A, C, and D came over, whereas fewer variants of lineage B came over in the initial migration(s). This is in contrast to the assertion made by the Wallace lab that only the four original founding haplotypes

crossed into the New World, and diverged thereafter. We suggest that there was divergence within the lineages (at least in A, C, and D) prior to crossing over into the New World. The implication of this is that the dates assigned to the coalescence time for each lineage are overestimates (since some of the divergence within each lineage occurred prior to entering the New World).

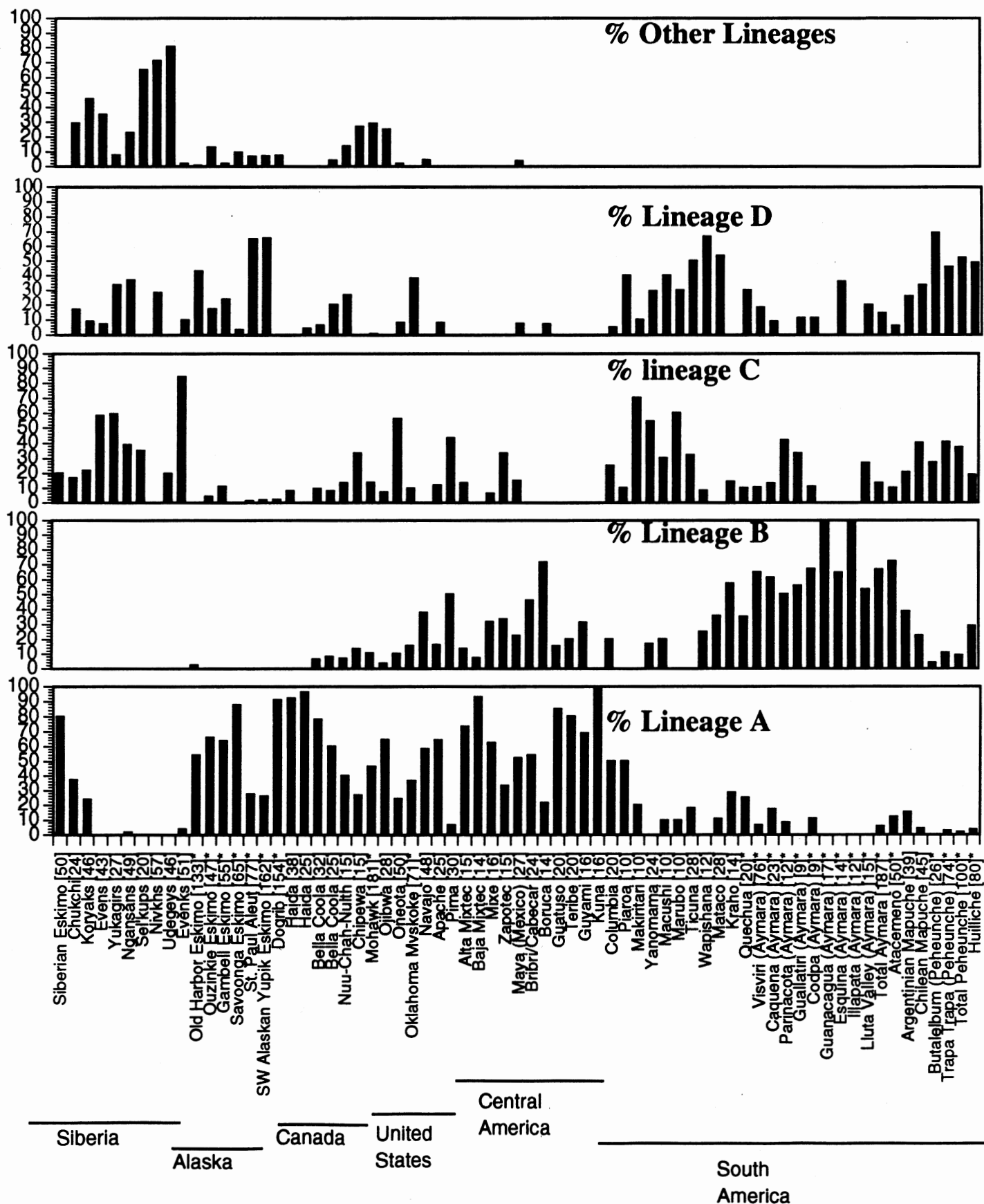
Merriwether (1993) sequenced over 250 Native Americans for the second hypervariable region of the D-Loop, (nucleotides 8-430) and confirmed that Amerindians cluster by founding lineage type (A, B, C, D) as determined by RFLP typing. Figure 2 consists of a subset of the data from Merriwether (1993) containing only Huilliche, Pehuenche, and Quechua Indians from Chile and Peru. Multiple clusters of lineages B, C, and D were observed that were not contiguous. All members within a cluster tended to be of one founding lineage group, AND from the same population. For example, we identified Clusters on our phylogenetic trees that were Huilliche-specific AND lineage D-specific, clusters that were Pehuenche-specific AND lineage D-specific, etc... We interpret such clusters as consisting of the founding lineage types for each population (represented at the nodal position of the cluster) and all the variants of that founding type which have arisen by mutation since that population was founded. This enabled us to estimate the number of founding haplotypes for each population.

In conclusion, we feel that the evidence from mtDNA data fits best with a model of a single migration containing multiple variants of all four lineages. This migration gave rise to the Amerinds, NaDene, and Eskaleuts. It certainly fits best with all three groups being descendent from the same parent population. Multiple migrations cannot be ruled out by our data, and the coalescent and divergence time data from the Wallace lab and the Ward lab provide some support for multiple migrations. In addition, Torroni et al. (1992) showed that there is a NaDene-specific polymorphism (an Rsa I 16349 site loss) which has not been identified in non-NaDene populations. It is not found in all NaDene populations, but may be good evidence for a common origin for the NaDene. Despite this, all four founding lineages types are so broadly distributed in the New World that we feel it can best be explained by a single migration and differentiation thereafter. Our sequencing data support this by showing population-specific clusters of each founding lineage type.

Acknowledgments: thanks to my co-authors Robert E. Ferrell and Francisco Rothhammer for their help acquiring the samples, providing laboratory facilities, and guidance. Thanks to the Wenner Gren Foundation and the W. M. Keck Center for Advanced Training in Computational Biology, University of Pittsburgh, Carnegie Mellon University, and the Pittsburgh Supercomputing Center for partial support of D. A. M..

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**Figure 1: Distribution of the four founding lineage types (and other types) in Siberia and the New World. This data for this figure is described in Merriwether et al (in press) and Merriwether et al (1994).**

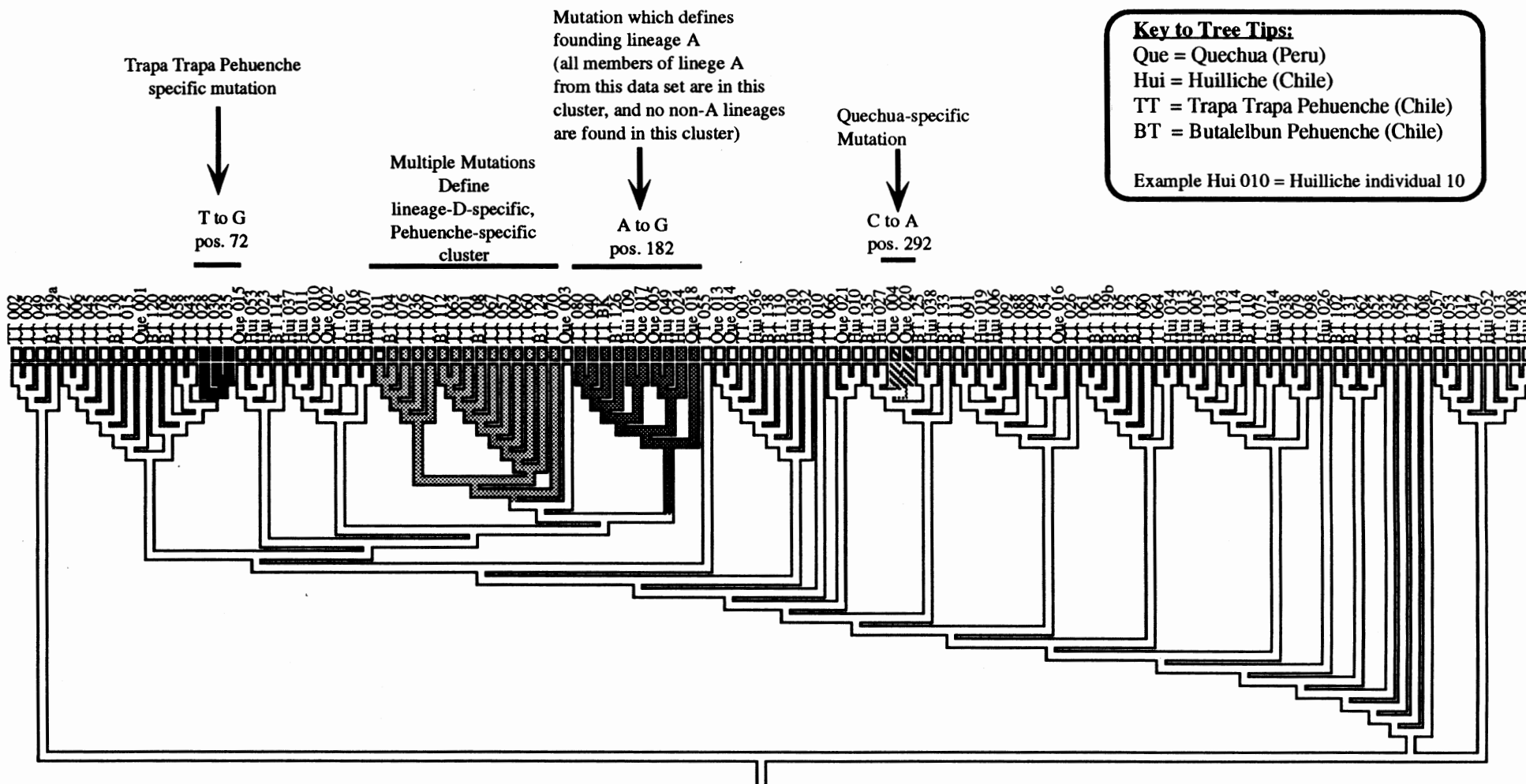


Figure 2: mtDNA D-loop neighbor-joining tree (nts 8-430) consisting of Huilliche, Pehuenche, and Quechua Indians from Chile and Peru. This tree is derived from a subset of the Merriwether et al (1993) dissertation.

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## Founder Mitochondrial Haplotypes in Amerindian Populations

Summarized from *American Journal of Human Genetics* 54 (1994), pp. 27-33 (with permission).

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### "Summary"

"It has been proposed that the colonization of the New World took place by three successive migrations from northeastern Asia. The first one gave rise to Amerindians (Paleo-Indians), the second and third ones to Nadene and Aleut-Eskimo, respectively. Variation in mtDNA has been used to infer the demographic structure of the Amerindian ancestors. The study of RFLP all along the mtDNA and the analysis of nucleotide substitutions in the D-loop region of the mitochondrial genome apparently indicate that most or all full-blooded Amerindians cluster in one of four different mitochondrial haplotypes that are considered to represent the founder maternal lineages of Paleo-Indians. We have studied the mtDNA diversity in 109 Amerindians belonging to 3 different tribes, and we have reanalyzed the published data on 482 individuals from 18 other tribes. Our study confirms the existence of four major Amerindian haplotypes. However, we also found evidence supporting the existence of several other potential founder haplotypes or haplotype subsets in addition to the four ancestral lineages reported. Confirmation of a relatively high number of founder haplotypes would indicate that early migration into America was not accompanied by a severe genetic bottleneck."

"Introduction". [Note: we skipped that as largely redundant - HF]

### "Materials and Methods" (page 28)

"Total DNA was extracted from blood samples drawn from 58 Mapuches, 38 Huilliches, and 13 Atacameños... The geographic location of these populations is as follows: Mapuche — Cerro Policía, 68°37'W, 39°10'S; and Aguada Guzmán, 68°58'W, 39°30'S; Huilliches — San Juan de la Costa 73°W, 41°16'S; and Atacameños — San Pedro [de Atacama - HF], 68°37'W, 22°S (fig. 1). Caucasian gene admixture was calculated through the ADMIX program (Chakraborty 1975), by using alleles of the ABO, Rh, Kell, Lutheran, and Kell system, with the allelic frequencies estimated by the MAXLIK program (Reed and Schull 1968). It was found to be <12% ± 0.03% for Mapuches and <5% ± 0.01% for Huilliches and Atacameños... ." [Technical stuff for the pros - HF]

## “Results and Discussion”

### “A-E and Compound Haplotypes”

“The four basic ancestral lineages detected by Torroni et al. (1992) in Amerindians are characterized by the gain of a *HaeIII* site at bp 663 (haplotype A); deletion of a 9-bp repeat corresponding to a small noncoding region lying between the genes for cytochrome oxidase II and TRNA<sup>L</sup> (haplotype B); loss of a *HincII* site at bp 13259 (haplotype C); and loss of an *AluI* site at bp 5176 (haplotype D). Table 2 shows the frequency of haplotypes A-D in corresponding to 21 different tribes of Amerindians. Ninety-seven percent of individuals showed one of the claimed four founding lineages, while 2.5% of the sample showed haplotype E (this haplotype is identified under the name “others” in Torroni et al. ...), and 0.8% of the sample showed a compound haplotype formed by the combination of two founder haplotypes.”

“Every individual not belonging to haplogroups A-D and not showing a compound haplotype is included in haplotype E. Torroni et al. (...) consider this haplotype a marker of Caucasian gene admixture, on the basis that haplotype E is very frequent (93%) in Europeans (Cann et al. 1987) and has been detected mainly in Amerindian populations with history of admixture. Haplotype E is also very frequent (75%) in Asiatics [sic -HF] (...) and Siberians (27%) (...). Therefore, it seems evident that the possibility of an Asian ancestry for some of the Amerindian haplotypes E cannot be ruled out. A clear example supporting this assumption is quoted by Torroni et al. (...); in a series of 10 Makititare aboriginals studied, the authors found one case (haplotype AM83) showing haplotype E in association with *AluI* and *DdeI* gains at bp 10397 and 10394, respectively, two RFLPs that are frequently associated with haplotype C. To explain these findings, Torroni et al. (...) propose a mutation reverting the loss of the *HincII* site at bp 13259 (haplotype C) to the original *HincII*<sup>+</sup> state (haplotype E). Although this hypothesis is plausible, it is also possible to assume an Asian origin for the Makititare haplotype E. In this regard it is interesting to mention that the analysis of blood groups in the six Mapuche individuals in whom our group detected haplotype E showed no indication of Caucasian gene admixture. Recently, haplotype E was detected in a pre-Columbian Amerindian mummy from a series of 50 pre-Columbian mummies studied by Stone and Stoneking (1993) (haplotype E is identified as N in table 2 of Stone and Stoneking 1993). This finding would confirm the Asiatic origin of some of the Amerindian haplotypes E.”

“Compound haplotypes — that is, the coexistence of two or more of haplotypes A-D in an individual — are found at low frequencies in Amerindian populations (...) and also in Asiatics (...) and Siberians (...). Since mtDNA does not undergo recombination, it must be concluded that compound haplotypes have arisen by parallel mutations, that occurred independently on the Asian and American continents. Alternatively, it is also possible to assume that compound haplotypes originated by parallel mutations in Asia and entered into America during the colonization of this continent. In this latter case, their low

frequency in Amerindians may indicate that they are in the process of becoming lost because of genetic drift.”

### “*HaeIII* 16517 bp Gain”

“Torroni et al. (...) have reported that all Amerindians in haplogroup B also exhibit a *HaeIII* gain at bp 16517. Thus far, the deletion of a 9-bp repeat in region V, with the gain of the *HaeIII* site, has been confirmed in a total of 297 Amerindians in whom the association of these two mitochondrial polymorphisms was assessed (...).”

“Conversely, haplotypes A, C, and D may or may not exhibit the *HaeIII* gain at bp 16517. We shall identify as ‘A<sub>1</sub>’, ‘C<sub>1</sub>’, and ‘D<sub>1</sub>’ the subset of haplotypes having the *HaeIII* gain, while ‘A<sub>2</sub>’, ‘C<sub>2</sub>’ and ‘D<sub>2</sub>’ will define the subsets lacking the *HaeIII* site. Table 3 summarizes the frequencies of each subgroup of haplotypes in Amerindians.” [Ed. note: we show none of their tables herein. - HF]

“Torroni et al. (1993b) propose a phylogenetic relationship between Siberian and Amerindian populations. Thus, these authors propose that A<sub>2</sub>, C<sub>2</sub>, and D<sub>2</sub> are founder Amerindian haplotypes, because of the fact that these subsets show high frequency in Siberians. On the other hand, A<sub>1</sub>, C<sub>1</sub>, and D<sub>1</sub> are assumed to result by mutations occurring in Amerindians and generating either the gain of a *HaeIII* bp 16517 site (A<sub>1</sub> and D<sub>1</sub>) or the loss of the *HaeIII* site with subsequent reversion to the original state (...).”

“The combination of A<sub>1</sub>, C<sub>2</sub> and D<sub>1</sub> subsets represents 30% of the total group of Amerindians having haplotypes A, C, and D. If we assume both a D-loop mutation rate of 30%/1 million years (...) and a pre-Clovis colonization time of 30,000 years ago, it can be estimated that  $1 \times 10^{-3}$  is the probability of mutation of a given nucleotide during the 30,000 years of colonization; thus it seems unlikely that 30% of the haplotypes have arisen by parallel nucleotide substitutions (lower estimates of the mutation rate in the D-loop region and shorter estimates of the period of colonization of America would decrease further the probability of parallel nucleotide substitutions). A loss of the *HaeIII* bp 16517 site with reversion to the original state is four times more frequent than a gain, because it results from a mutation in any of the four nucleotides forming the *HaeIII* site, versus the single transition required to produce the gain of the site. Thus far, neither in Asiatics (...) nor in Amerindians has a single case of haplotype B with lack of the *HaeIII* site at bp 16517 been reported. Therefore, if we assume that C<sub>2</sub> cases arise by reverse mutations, we have to explain why these mutations do not occur in haplotypes B. From the above considerations it seems that the alternative that best accounts for the subsets found in groups A-C is the assumption that A<sub>1</sub>, A<sub>2</sub>, C<sub>1</sub>, C<sub>2</sub>, D<sub>1</sub>, and D<sub>2</sub> are all founder maternal lineages.”

### “D-Loop Sequencing”

“The determination of the nucleotide composition of the D-loop has been also used to characterize the haplotypes of Amerindians. Four groups of investigators have employed this approach and have sequenced equivalent regions of the D-loop,



allowing us to compare the results obtained by each group (...)"

... [Ed. note: we skip 3 paragraphs of technical argument. - HF]

"In summary, although all authors sequencing the D-loop detect the existence of three or four haplogroups, the comparative analysis indicates the presence of five haplogroups and two subsets of haplogroup I. All the transitions listed in Table 3 are also present in Asiatics (Table 5). When the low frequency of haplotypes I and IV in Asiatics is taken into account, it seems logical to assume that the lack of haplotype II in this ethnic group may be due to a combination of the low frequency of the haplotype and the small size of the sample analyzed (101 individuals)."

#### "Concluding Remarks"

"The characterization of haplotypes in New World populations has been, at first sight, more rewarding than the attempts to date the origin of Amerindians. There is evidence suggesting the existence of four (A-D) or five (I-V) ancestral haplotypes in most Amerindians. However, what it is not yet clear is [sic - HF] how many founding haplotypes should be (1) both widespread within Amerindians and shared between tribes, (2) central to the branching of their haplogroup in parsimony trees, and (3) still be present in the Asiatic populations. These conditions were enunciated while having in mind the properties of haplotypes A-D. In fact, subgroups A<sub>1</sub>, A<sub>2</sub>, C<sub>1</sub>, C<sub>2</sub>, D<sub>1</sub>, D<sub>2</sub> meet the above-mentioned parameters. There are, however, other cases of potential founder haplotypes, cases that should not necessarily meet the above requirements. If a founder Amerindian haplotype exists at low frequency in Asiatics, its discovery will depend on the size of the sample studied. Furthermore, the haplotype may have existed in the original Asiatic population that colonized America and may have become lost in extant Asiatic populations. Haplotype II (...) is a case in point. Both transitions defining this haplotype are found in Asiatics; however, the combination of the two transitions in single individuals (haplotype II) is found in 28% of Amerindians but has not yet been detected in Asiatics."

"The number of founder Amerindian haplotypes is a problem at the center of an unresolved dispute. According to Torroni et al. (1993b), the colonization from Asia into the American continent was accompanied by a severe bottleneck that markedly restricted the number of maternal lineages entering the New World. Ward et al. (1991) and Horai et al. (1993) propose an opposite view. The genetic diversity detected in Amerindians is, according to these investigators, too extensive and, consequently, does not support the hypothesis of the genetic bottleneck. Confirmation of the presence of more than four founder haplotypes in Amerindians would lend additional support to the positions of Horai et al. and Ward et al." [Ed. note: both were reported previously in MT. - HF]

"Haplotypes A and D are found not only in Asiatics, but also in a low number of Caucasians (Cann et al. 1987). Haplotype B has been found in Asiatics and also in some Nigerians (Merriwether et al. 1993). On the other hand, haplotype C has not been reported in Caucasians or Africans

thus far. This finding seems to confirm the Asiatic origin of Amerindians; yet, the ancestral Asiatic population(s) from which the Amerindians derived is a matter of speculation and debate. Torroni et al. (1993b) proposed that Amerindians in haplogroups A, C, and D derive from Siberian populations located close to Beringia. Moreover, since these populations do not show the haplotype B, it is speculated that carriers of haplotype B originated in southeastern Siberia and migrated into America at a time later than the migration carrying haplotypes A, C, and D but earlier than the migration that gave rise to Nadene (...). Shields et al. (1993), on the other hand, propose that Siberian populations surrounding the Bering area have a more recent origin than do Paleo-Indians. Therefore, it is proposed that all Paleo-Indian haplotypes derive from southeastern Siberian populations that came into America some 12,000 years ago."

"The possibility of identifying the ancestral founder haplotypes of Amerindians has been questioned by some investigators. Chakraborty and Weiss (1991) reanalyzed Schurr et al.'s (1990) data on three Amerindian populations and proposed that mtDNA is in mutation-drift equilibrium and that it is not possible to identify ancestral lineages. Additional arguments casting doubts on the models derived from the existence of founder Amerindian haplotypes have been put forward recently by Szathmari (1993)."

"Despite the above-mentioned criticisms, it seems clear that the mtDNA tool has provided reliable information with which to reconstruct part of the history of Native Americans. Yet, many other parts of this history remain unresolved. At this time it seems too optimistic to think that the mtDNA of extant Amerindian populations will serve either to resolve all doubts about the origin of Amerindians or to reconstruct the evolution of primitive populations, some of which may have become extinct because of epidemics, wars, and the forced resettlements that occurred during the conquest of America. Perhaps the use of new molecular tools, such as specific regions of the Y chromosome, in combination with the mtDNA method and the analysis of mtDNA from ancient human remains from America and Asia, will shed more light on the evolution of the indigenous populations of the New World."

[Ed. note: the "Acknowledgments" and "References" sections are not included here. Most of the references overlap with Cann's or Merriwether's. - HF]

Postmortem comments: It is extraordinary how bright young scholars will sometimes surrender to criticisms by senior scholars. In looking for ways to find another bright future, now being unsure of themselves, the authors look to more technology, more high tech stuff. Yet it is almost as extraordinary that they say naught about nuclear DNA or a bit more old-fashioned serum protein + red cell genetics (roughly blood group stuff). They also seem to lack any hope of help from archeology or historical linguistics. But some of us think their article has made a serious contribution, e.g., Becky Cann just below here. In the next issue of *Mother Tongue*, we will reproduce a review of this article by Andy Merriwether; plus his article on ancient Amerind mtDNA.



## mtDNA AND NATIVE AMERICANS: A SOUTHERN PERSPECTIVE

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[Rebecca Cann wrote this invited (guest) editorial for the *American Journal of Human Genetics* (*Am. J. Hum. Genet.*), vol. 55, 7-11, 1994. It was written in April, before Andy's article. We have altered or abridged her editorial, which is presented here without most of its bibliography and references with her permission but with new footnotes. - HF]

### "Introduction"

"Bailliet et al.'s (1994) article 'Founder Mitochondrial Haplotypes in Amerindian Populations,' in this issue of the journal, magnifies the importance of understanding the genetic links between modern Native American populations, their ties to past populations, and their connections to the aboriginal people of Asia. In particular, human geneticists are now poised to break the circular use of linguistic, dental, and archeological evidence and accurately dissect the last continental expansion of anatomically modern people. Contrary to expectation that the slow accumulation of evidence from enough loci would eventually resolve contradictions, we see instead that the key to unambiguous results is, once again, the careful attention to how and where populations are sampled ..."

"Although a number of landmark papers in anthropological genetics using new mtDNA markers have appeared in the past 3 years (...), the interpretation of these data sets have not been straightforward. At face value, any date 40,000-15,000 years before present (ybp) is consistent with the arrival of the first Native Americans. Not only did authors publishing about aboriginal Americans disagree on the number of migrations, they also parted company on when people began moving, and from where."

"Readers here may be relieved to know that their frustration in understanding the extensive dialogue on Native American genetic diversity that has taken place between research laboratories is widely shared among anthropologists and linguists as well (...): 'one could argue, of course, that there have always been foolish people and there always will be' (Fleming 1993, p. 17). If the questions did not interest so many at a variety of levels, we might simply ignore the lack of consensus." [Note: I had to include that reference, obviously. - HF]"

"What were the genotypes of the first Americans, their time of arrival, or their genetic population structures? Where exactly did they stem from, and how fast did they expand? Most human geneticists have believed that there were at three different migratory waves from north-central Asia, corresponding to Paleoindians of North and South America

(roughly corresponding to Amerind), Na-Dene peoples now largely restricted to North America, and the Aleut-Eskimo populations of the far north who now occupy both sides of the Bering Straits (...). Genetic, linguistic, and morphological data on Native Americans were summarized for a general audience during the mid 1980s in a widely cited multidisciplinary paper (Greenberg et al. 1986). Instead of development of diversity in situ, these authors placed the major origins of diversity in three different founding groups that took separate evolutionary trajectories in time and space. According to their model, three groups of migrants were significantly divergent in language, morphology, and genetic markers before they crossed Beringia and extended their range into the Americas. The time scale associated with first colonization was open but was assumed to be related to Clovis-age artifacts <13,000 years old. Owing to the relatively recent appearance of RFLP and DNA sequence data, the authors relied on more readily available protein polymorphism studies."

"Although there has been a significant amount of dissension among linguists regarding Greenberg's (1987) startling classification of American language groups into only three major phyla, he is also supported by many specialists. The close correlation between some linguistic groups and their gene pools (...) has meant that geneticists, on the whole, have been less critical of Greenberg's ideas. One break with this tradition among geneticists has been the recent paper of Ward ..., which questions the equivalence of demes with linguistic units in the Pacific Northwest. I also refer readers to Szathmary's (1993a) excellent summary of the history of anthropological issues raised by previous American colonization models — and of how new genetic information was supposed to support or refute those models."

### "The Beginning of the Dialogue"

"Early work with maternally inherited mtDNA RFLP patterns in Pima, Maya, and Ticuna Indians was interpreted to support the suggestions of a significant population bottleneck in the peopling of North America, with a secondary barrier developing in Central America that partially isolated the populations of South America. This supposedly accounted for the different frequencies of known lineage clusters found there (...). If the bottleneck in Beringia or North America was prolonged, genetic diversity might be purged and surviving modern lineages could appear to trace to a single geographic homeland. The task was to clearly identify the homeland."

"In the Amerind donors sampled, only four major RFLP haplotype clusters (A-D) were recognized by these authors; and the implications of a dramatic founder event for epidemiological genetics were profound. In passing through the Arctic filter, some Old World genotypes and associated diseases might have been lost, but others could have been accentuated. Diabetes, hypertension, alcoholism, or any other current public health problem that had genetic as well as socioeconomic components might be shown to have simple genetic correlates in indigenous peoples. Genetic screening for individuals potentially at risk could conceivably be cheap as

well as comprehensive. In trying to solve the colonization puzzle, it looked as if researchers might also score a major advance for genetic epidemiology."

"Torroni et al. (1993b) have recently suggested that the homeland of Amerinds is in eastern Siberia, on the basis of comparing 10 aboriginal populations in Siberia and the Russian Far East with Native Americans. Three major mtDNA groups are present in this region. The authors acknowledged that modern Siberians lack a maternal genetic cluster (termed 'B'), now present at highest frequency in South Americans, Pacific Islanders, and Indonesians (...). They presume that this lineage came into the Americas as a second, later wave of Amerind migrants (Torroni et al. 1994)."<sup>1</sup>

"In fact, the B lineage cluster is present along the Pacific coast of North and South America. It was also reported to be present in an ancient Colorado Paleoindian mummified donor dated 8,000 ybp (...). Now, Bailliet et al. document that this lineage cluster, as well as four others, can be found in unadmixed Argentine Native Americans. Researchers are asking, just how robust can the Siberian-model ancestor for all Amerinds be — and just how robust can the time scale associated with timing Paleoindian entry into North America be — if we cannot yet agree on the number of major lineage groups present in the initial colonization wave?"

"On a theoretical front, disputes arose around the idea that Native Americans had ever undergone a significant genetic population bottleneck (...). This challenge gained momentum when mtDNA hypervariable sequences were reported for a single Amerind tribe (Nuu-Chah-Nulth) [Ed. note: that's Nootka - HF] from the Pacific Northwest. The authors of this independent study failed to support the limited diversity/restricted-origin model (...). Instead of only four haplotypes, that study found 28 discrete maternal lineages in just 63 donors. The confusion between haplotypes as one lineage and haplotype clusters as seen in sequencing leads to a resolution problem. By focusing on a single segment of the mitochondrial hypervariable control region in one tribe, those authors discovered maternal diversity that was equivalent to ~62% of that found in modern Africans and to ~81% of that present in urban Japan. The Amerind maternal lineages, moreover, traced back to a coalescent female who was projected to have lived ~60,000 ybp. Far from supporting the hypothesis of a genetic population bottleneck in the founding of Amerindians, this study argued that comparatively large groups were involved in the colonization of the New World, on the basis of both the large number of Nuu-Chah-Nulth lineages surviving today and the implied effective population size."

"Critics countered that this tribe was merely an amalgamated group of epidemic smallpox survivors (aren't we all!) with questionable linguistic affinities, not a local population equivalent to previously tested groups. According to others, multilocus electrophoretic or RFLP patterns from nuclear loci would resolve issues in human population expansion that mtDNA could not (...). Reasoning that mtDNA samples represented only a single locus with large stochastic variation, they asserted that few definite results would ever be expected from using such a system. This criticism might have

intimidated some people, but it should also be noted that the most recent synthetic summary of human population genetics from 29 polymorphic nuclear loci in 26 populations with 121 alleles (and no missing comparisons, making it unlike the presentation of Cavalli-Sforza et al. 1988) could not even state, with statistical significance, whether modern Eskimo populations are more closely related to North American or to South American aboriginal groups (Nei and Roychoudhury 1993)! "<sup>2</sup>

"New hypervariable mtDNA sequencing studies by ... fully support the view that Amerinds have an ancient maternal coalescent point, with many genetically distinct lineages. They can also be interpreted to show that most of the identified lineages correspond to one of four major clusters in phylogenetic analysis, vindicating previous RFLP surveys. Each cluster has a relatively deep coalescent, predating the Clovis artifacts associated with Paleoindian expansion. These studies, however, intensify the mystery both of migratory waves and of the associated time scales. The statistical power of the phylogenetic analysis in all cases is low, because of the large number of lineages examined and the relatively few number of substitutions. Bailliet et al.'s new study shows that the evolutionary patterns of Amerinds in the south had not yet been fully resolved by sequencing of hypervariable domains. High resolution is, unfortunately, the key to understanding a time scale, as well as to modeling the process of colonization."

"First, Bailliet et al. eliminate, once and for all, the idea that a severe population genetic bottleneck took place in the process of continental colonization in the New World. They chose southern and geographically distinct representatives of the Amerind language phylum, according to the Greenberg classification. In focusing on three Argentine [sic - HF] tribal populations (Mapuche, Huilliches, and Atacamenos) having minuscule to no evidence of genetic admixture<sup>3</sup> with Europeans or African ethnic groups, they roughly doubled the number of new maternal genetic lineage clusters. These lineage groups are present in modern Asian populations and should have been present in the Americas, if modern Asians represent the logical source for most of the lineages that made up the gene pool of the first Americans. In fact, some of these lineages had been uncovered by previous workers but had been dismissed as either probably not authentic or enigmatic. Lineages that do not clearly fit the simple pattern (ABCD) are present in these indigenous peoples. Forced to confront the fact of differential lineage survival, we should have predicted as much from the drop-out of the B lineage cluster in some North Americans and Siberians. However, why is the B cluster always lost?"

"A full 10% of the Mapuche would be considered 'authentic' by the criteria that some assert should be used to recognize founder lineages. These individuals do not fit the predicted pattern (ABCD) and yet are present in a tribal group that, by geography alone, could represent some of the earliest arriving Amerind colonists. Owing to both the nature of the mutations uncovered and the rate of mutation for the hypervariable region sequenced, it is unlikely that >30% of the lineages discovered in these three tribes arose in parallel in both the Old World and South America."

"Bailliet et al. ask us to consider the effects that epidemic diseases, forced relocation and displacement that accompanied both indigenous and European expansion of dominant cultures, and active warfare would have had on the aboriginal American gene pool. Why shouldn't some groups be characterized by social structures that lead to high lineage extinction or explosive bursts of local expansion? Wouldn't nomadic hunters be expected to experience higher rates of transient lineage extinction than would settled agriculturists or local foragers? The notion that founding lineages will always be widespread and shared between tribes, will always be present in modern Asiatics, and will occupy a basal position in phylogenetic analysis may be considered unrealistic in light of present knowledge. In addition to this, what would be the consequences of a recent explosive burst of more closely related lineages with extensive gene flow?"

#### "Limited Resolution: Now Possible"

"Anatomically modern people might have successfully entered North America through Beringia anytime in the past 100,000 years, during the Wisconsin glacial (...). If they came by land, the Alberta corridor, an ice-free route of dispersal between the Rocky Mountains and Hudson Bay, is estimated by Wright to have been open during the period of 55,000-18,000 ybp, to have closed for ~6,000 years during the late Wisconsin glacial, and to slowly reopen as it is now. This corridor to the south also roughly corresponds to a time when the Beringian land bridge existed (65,000-13,500 ybp)."

"On the basis of estimates of productivity and available resources, it has been suggested that the time when it was best to actually cross Beringia was 40,000-30,000 ybp (...). Winter ice before 65,000 ybp and after 13,000 ybp would also have allowed people to move freely, according to seasonal constraints. Migrants would still have to trek across Siberian and Alaskan Arctic deserts and/or pass through bogs, but it is clear that the real time period of potential dispersal is enormous, as was the home range that founding tribal populations might have occupied."

"Archeological evidence under such conditions should be rare, as populations densities would be expected to be very low. Once in North America, colonizing groups could go farther inland or follow the coast south. The coastal route is considered high risk, because dangerous glaciers were thought to block clear passage to the south. However, with our knowledge of both the antiquity of the use of water craft and the plentiful supply of marine resources, as well as of the emotional familiarity of aboriginal peoples from southern Asia with dangerous marine environments and their mammals, coastal passages should probably be reconsidered as a viable alternative. The coastal route is a key factor in asking whether Amerind populations cut off from this region by the ice corridor were effectively isolated from tribes to the south, as well as from the coast, for 6,000-10,000 years. Pacific populations using this route of entry into the Americas might well show a different suite of genotypes, such as those in the B cluster, if they were in migratory equilibrium with coastal foragers now

displaced by modern Siberians."

"Undisputed archeological evidence of human occupation in the Americas is confined to the late Wisconsin glacial 23,000-10,000 ybp. Much of this evidence is <15,000 years old, and some of the oldest sites are found in South America, such as Monte Verde in south-central Chile and Pachamachay cave in highland Peru (...). For every advocate of the north-Asian/late-Wisconsin stage/three migration-wave model, it is possible to find a reputable researcher who supports, instead, a single sustained population pulse from a heterogeneous and predominantly north-Asian pool, now largely incorporated into modern cultural units. Can we dissect a one-wave from the many possible wavelets and infer their arrival? If the level of migration between groups, postexpansion, is low (<10 females/generation), then simulations suggest that we can (Harpending et al. 1993)." [See *Mother Tongue* 21, pp. 43-49. - HF]

"Geneticists should now consider a provocative idea that was recently proposed in order to focus debate on the early/late colonization models (...) to help predict patterns associated with human use of an immense, uncharted landscape. In Beaton's view,<sup>4</sup> one should contrast the world of transient explorers with that of estate settlers, who will have different demographic characteristics. The very slow or nearly stationary growth rate of high-mobility transient exploring groups should lead to rapid extinction of genetic lineages. In contrast, the high fecundity low-mobility settlers, still maintaining ties with their home groups and periodically exchanging mates, should show minimal lineage loss. These predictions will be familiar to those following the Rogers and Harpending (1992) pairwise-difference projections for population comparisons, as recently applied to both empirical and theoretical populations (...)."

"The observation that many Amerinds, especially in the north, have lost the B lineage cluster as well as other unrelated clusters can have a number of explanations. First, as Torroni et al. (1994) imply, we could simply have a second migratory wave of settlers carrying the B cluster, which comes in after a major expansion of most Amerind groups. The time of the Amerind expansion could be early or late. The absence of B lineages in Beringia today says that, if this expansion were late, then the second expansion group was also recently displaced by a modern Siberian population, more closely related by lineage frequencies to the original founders. The fact that B clusters are present at high frequency in the south in a presumed ancestral group contradicts this simple model."

"Second, North American Amerinds could be a second wavelet of the huge Amerind wave that began >20,000 ybp. A blockage of the Alberta corridor isolated Amerinds to the south, who continued to expand demographically, with minimal lineage extinction. Many lineages were lost in their northern cousins, who have secondarily expanded, after lineage loss, with the reopening of the corridor. This hypothesis accounts for some early sites in South America, higher genetic complexity there, and relative isolation between Amerind tribal groups as judged by traditional population genetic parameters. A prediction of this model is that Amerinds should be clustered

into linguistic sub-phyla by their retention of the mtDNA B and other lineage clusters (Alberta south), versus cluster ACD (Alberta north), and that these subphyla would now show tighter linguistic/genetic<sup>5</sup> affinities. Lower diversity in the B cluster contradicts the prediction that B is an early colonizing cluster.”

“A third possibility includes the idea that the lineage B cluster is contributed either by a continual trickle of colonists using the coastal route or by direct contact across the Pacific ocean. Amerindian groups most likely to be in migration equilibrium with such a source of lineages are those closest to the tropical west coast. The B cluster of lineages have a coalescent of almost 30,000 years, so they might represent a separate source population isolated in the south of Asia. A coastal route in equilibrium along the entire Pacific Rim does not yet account for the geographic gradient that is seen in B lineage frequencies, which are highest always in the south. Pacific voyagers could have contributed this lineage separately to the Americas, without ever going through Beringia. A prediction of this model is that the B lineage cluster should be seen as intrusive archeologically, confined to a time scale when we know that active voyaging was taking place in Remote Oceania. This time period corresponds to the spread of the Lapita cultural complex and has an antiquity of only 6,000 years (...). Thus, the observation of an 8,000-year-old Paleoindian with a B lineage might seem to invalidate this model. However, B is a diverse lineage cluster, and, if it is retained ancestry versus intrusive, unique mutations found today in Remote Oceanic lineages should be missing in this sample when it is subjected to more extensive analysis.”

#### “Conclusion”

“The inadequacies of the archeological record require us to face the facts that ‘traditional’ evidence (i.e., a recurrent pattern of stratigraphic sequences based on radio-carbon dates in defined cultural settings) supporting the idea that the Americas were colonized >14,000 ybp is not strong. A simple, late expansion of three into the New World is dead, however. Active exploration and documentation of archeological sites in the Americas is continuing, and at any moment the time depth for first occupation may change. Massive disease epidemics that followed cultural displacement were thought to have plagued aboriginal population genetic reconstructions, and they may not have been severe for Amerinds (Stone and Stoneking 1993).<sup>6</sup> South American populations will have central importance in quantifying loss of diversity versus groups in the north, because of the opportunity to check for lineage extinction against skeletal remains. We will never be able to recover archeological sites lost to coastal flooding, shopping-mall development, modern agricultural practices, or repatriation of stolen human remains. As limited as our opportunities may be, reconstructions of past human population diversity that are based on inferences of DNA sequence variability are the only independent way to scientifically approach the questions of Native American genealogical relationships.”

[Thus ends Becky Cann’s guest editorial.]

Post-prandial comment: After such a good lunch, a few commonplace things and one extraordinary thing need to be said. First is that the surprising links or happenstance similarities between biogenetic factors in South America and Polynesia have been observed at least as early as 1954 in A. E. Mourant’s famous book *The Distribution of the Human Blood Groups*. Secondly, the Polynesians are simply too late archeologically in the eastern Pacific to be serious contenders for a source population for the native South Americans. The latter share some common human traits with the Polynesians but in Gamma Globulin, at least, which is a critical marker gene, the famous Austronesian *fab* is virtually absent in South America. It reaches very high percentages in many Polynesian and Micronesian populations. I propose that the famous Austronesian sailors could have reached the Americas frequently but had no serious biogenetic impact on them. Third, resident populations of greater Japan lack most of the specialized Gamma Globulin haplotypes, just as South Americans do, and they occupy a nearly perfect place for long distance sailing by short spurts all the way across the *North Pacific* and then down the coast towards California, Mexico, and Colombia.

Fourth, most extraordinarily, Andy Merriwether, Becky Cann, and I seem to agree that the genealogies proposed by mtDNA freaks are not the same as cladograms produced by biologists or most physical anthropologists or family trees produced by linguists. You have great difficulty getting statements like “the Iroquois are closer to the Cherokee than they are to the Sioux but the three of them form a unit as opposed to the Zuni” or words to that effect. So in our testing of the detailed Greenberg taxonomy against the mtDNA results, we find we cannot compare them! At least not yet. The reason may be that the mtDNA results show what lineages there are in a people and how various lineages relate to each other whether *inside or outside* the people (population). In a cladogram, we classify populations or ethnic groups in relation to each other; in a family tree, we do the same thing for dialects and languages. Yo, long rangers! We need some help thinking about this! Maybe best from social anthropology? Yo, Stephen Tyler, how about thinking of the “mtDNA-grams” as matri-sibs or matri-phratries?

#### Notes

1. Editor’s footnote. Because we published both Torroni articles referred to (in *Mother Tongue* 21, pp. 50-54) we must refer you all to his latest which we did not know about. A. Torroni, J. Neel, R. Barrantes, T. G. Schurr, and D. C. Wallace (1994). “Mitochondrial DNA ‘clock’ for the Amerinds and its implications for timing their entry into North America.” *Proceedings of the National Academy of Science USA* 91: 1158-1162. - HF

The Colorado mummy referred to in the next paragraph was reported to you in *Mother Tongue* 21 too. Maybe “Iceman of the Rockies” would be better than mummy, since he

was frozen.

2. This is an important work too long neglected by us. See M. Nei and A. K. Roychoudhury (1993). "Evolutionary relationships of human populations on a global scale". *Mol. Biol. Evol.* 10:927-943. Nei is a senior biogeneticist at Pennsylvania State University and an influential scholar.

3. The donor ethnic groups were from Argentina and Chile equally. The degrees of Caucasoid admixture were calculated to be <12% for Mapuches and <5% for Huilliches (south Chile) and Atacamenos (north Chile). One may doubt that 11% is minuscule, but Mapuche at least have <02% of *fb*, the Gamma Globulin marker gene for Caucasoids. If not minuscule, it's very small.

4. Since the text requires the reference, it is to J. M. Beaton (1991). "Colonizing continents: some problems from Australia and the Americas." In T. M. Dillehay and D. Meltzer (eds.), *The first Americans: search and research*. CC Press, Boa Radon, pp. 209-230.

5. Lest linguists be confused here, her meaning is genetic linguistic in relation to biological genetic (biogenetic) taxa or units. A gentle reminder to our colleagues in biology that the term genetic and the inheritance it implies is *not* owned by biology. In fact, linguists had such a notion long before Darwin. Greenberg's original Amerind taxa were twice as numerous in the south.

6. Editorial note: is it possible that Stone and Stoneking do not know about the enormous loss of population in native America in *post-Columbian* times? Millions and millions died, or so it has been calculated by historians, and much of it due to diseases. See A. C. Stone and M. Stoneking 1993. "Ancient DNA from a pre-Columbian Amerindian population". *American Journal of Physical Anthropology* 92:463-471. - HF.

## NEW HOMINOID FOSSIL EVIDENCE FROM VIETNAM

Geoffrey Pope, Milford Wolpoff, and Pardner Hicks are not alone in proposing serious alternatives to the more or less accepted general ideas that modern human beings descended from earlier hominids, most particularly *Homo erectus* of the western Old World, and spread around the world eventually replacing and/or absorbing other hominids. Jeffrey Schwartz of University of Pittsburgh is on record as proposing, in a most general sense, that the human descent line leads back to orangutans or at least that we are closer to orangutans than we are to chimpanzees and gorillas. While his theory has not commanded general support, it has not been laughed out of court either. Recently, he and some colleagues did some more digging and re-evaluating of fossil finds from Vietnam. It was reported previously in the press, but we regrettably missed it. What we have to report here is Jeffrey's own analysis of what they found there. Not only is it interesting but it flatly contradicts the New York Times statement (see below) that no *ape* fossils have been found — ever anywhere.

There are two reports, the one a formal publication of the American Museum of Natural History (New York) which came out in January 1994 and was presumably reported in the *New York Times*. The second report is a follow-up after some new research; it will be published separately. Unfortunately, while the reports refer to sources, there is no bibliography of the references given. It is too late in our publication cycle to rectify this matter.

First report. "A Diverse Hominoid Fauna From the Late Middle Pleistocene Breccia Cave of Tham Khuyen, Socialist Republic of Vietnam," by Jeffrey H. Schwartz, Vu The Long, Nguyen Lan Cuong, Le Trung Kha, and Ian Tattersal. January 1994. *Anthropological Papers of the American Museum of Natural History*. No.73, 11 pages, 6 figures, 2 tables. \$2.10 per copy. The abstract follows:

"The cave Tham Khuyen in Lang Son Province, northeastern Vietnam, has yielded a large mammalian fauna of probable late middle Pleistocene date. A series of isolated hominoid primate teeth, formerly allocated to the extant orangutan *Pongo pygmaeus*, has recently been reexamined and found to represent more than one species. These specimens are described in detail in this paper and are analyzed as follows. Some of the teeth are indeed clearly identifiable as those of *Pongo pygmaeus*, but the majority appear to belong to a species related to the orangutan but not identical with it. A few teeth are distinct from either of the above, both in size and morphology, and are interpreted here as representing a previously undescribed genus and species of a large-bodied hominoid. In addition, a few teeth are regarded as indeterminate at present. With the recognition of this multiplicity of hominoid species at Tham Khuyen, it is evident that the large-bodied hominoid fauna of middle Pleistocene Vietnam was considerably more



diverse than formerly supposed, including *Gigantopithecus blacki* and *Homo sp.* in addition to the species noted above."

[Note to the general reader. The terms "hominoid" and "hominid" are anthropological conventions nowadays, i.e., more or less fixed terms. The first is a man-like creature. The second is a human or at least a hominoid in the direct line to moderns. - HF]

The second report by the same authors (without Le Trung Kha) will be: "A Review of the Pleistocene Hominoid Fauna of Vietnam (excluding Hylobatidae)." Same publication outlet. n.d.

[Note: Hylobatidae refer to those great aerialists — the gibbons]

[Note: we skip the Abstract which is similar to that above.]

"Prior to the research reported by Schwartz et al. (1994) it had been believed that all fossils of large-bodied hominoids from Vietnam could be subsumed within *Pongo pygmaeus*, *Homo sp.* and *Gigantopithecus blacki*. At that time reappraisal of specimens from Tham Khuyen Cave made it apparent that at least two additional distinctive morphs existed in that assemblage, and these have been named here as a new genus and species of hominoid, *Langsonia liquidens*, and a new species of *Pongo*, *P. khaei*. In this study, we have broadened our assessment to cover large-bodied hominoid specimens from all significant Pleistocene sites in Vietnam, and have noted yet more diversity among the fossils previously identified as orangutans or hominids."

"For example, in the sample of isolated teeth from Than Om there are some teeth that do not fit comfortably in any existing taxon, including those named in this paper. Two lower molars (77.TO.o.7<sup>b</sup>.v.38 and 75.TO.39), although orangutan-like in degree of enamel crenulation, are less so in having more restricted occlusal basins and being more narrowly rectangular in occlusal outline. Even more singular are a lower molar (77.TO.v.01.39) and an upper molar (75.TO.H), which are smaller than associated orangutan material and which are distinctly non-orangutanlike in morphology (in contrast to the small specimens from Hang Hum). Beyond noting that these are teeth belonging to small, thick-enameled non-hominid hominoids, on current evidence we are unable to determine the affinities of these molars."

"The cave of Hang Hum has also yielded three teeth whose affinities we are unable to resolve with certainty, although we have considered them "default hominids" above. Two are lower left molars, and unfortunately bear illegible catalog numbers; one is an upper molar, HH79. These teeth were initially identified as *Homo sapiens*, but their large size places them outside the range of our own species. The two lower molars compare most closely in size and morphology with those of *Homo erectus* from Zhoukoudian. The third tooth is too worn to permit similar comparison. If the lower molars are indeed those of *Homo erectus*, this would suggest a

remarkably late survival of this species in Vietnam. Additionally, two premolar teeth from Hang Hum (HH 7 and 128) are distinctly those of *Homo sapiens*. Additional material is needed to resolve the questions raised by these specimens; it is thus particularly unfortunate that the Hang Hum site has been inundated by an artificial lake."

"Tham Khuyen has also yielded specimens that are difficult to interpret. Two upper molars (TK 65/50 and 53) are distinct in both size and morphology from the *Pongo pygmaeus* and *Pongo khaei* specimens identified at this site. In general size and shape they most closely resemble the upper molar from Tham Om (75.TO.H) characterized above as that of a small, thick-enameled non-hominid hominoid of uncertain affinities."

"Apart from the adjacent cave of Tham Hai, from which only a single tooth is known, Tham Khuyen is the only site to have yielded fossils of *Pongo khaei*, which is far more abundant at this locality than any other hominoid, including forms of *Pongo pygmaeus* which compose almost the entire hominoid fauna at the other sites. Tham Khuyen is also the only site in Vietnam to yield definitive — if very sparse — evidence of *Gigantopithecus blacki*. Only one other Vietnamese site, Tham Om, contains even presumptive evidence of *G. blacki*, in the form of a single excessively worn upper molar. If the Tham Om specimen indeed represents *G. blacki*, this indicates a rather remarkable outlying occurrence, since the species is otherwise only known from China and the region of Vietnam immediately adjacent to the Chinese border, while Tham Om, some 350 km to the southeast of Tham Khuyen, is the most southerly of the important Vietnamese hominoid sites."

"*Pongo pygmaeus* is without question the hominoid that is most ubiquitous and most abundantly represented in the current record of the Vietnamese Pleistocene. It is the only hominoid that occurs at all significant cave and rockshelter sites, and it is frequently the only one. However, the Vietnamese fossil populations vary greatly among sites, the total variation greatly exceeding that seen in extant orangutans. At present, most hominoid systematists recognize several subspecies of *Pongo pygmaeus* in extant and fossil faunas. These are:"

"1. *Pongo pygmaeus pygmaeus* (Linnaeus, 1760), for the extant orangutans of Borneo."

"2. *Pongo p. abelii* (Lesson, 1827), for the extant orangutans of Sumatra."

"3. *Pongo p. palaeosumatrensis* Hooijer, 1948, for isolated Pleistocene fossil teeth from sites in Sumatra that are larger than those of *P. abelii*."

"4. *Pongo p. weidenreichi* Hooijer, 1948 for isolated Pleistocene fossil teeth sites in southern China and Indochina that are yet larger than those of *P. p. palaeosumatrensis*."

"In our recent review of the hominoids from Tham Khuyen (Schwarz et al., 1994) we noted that the isolated teeth of *Pongo pygmaeus* from that site fell within the size range quoted for the large subspecies *P. p. weidenreichi* by Hooijer (1948). There seems to be no reason to exclude the Tham Khuyen and Lang Trang orangutans from *P. p. weidenreichi*;

and doing so, moreover, both greatly increases the hypodigm of this extinct taxon and provides evidence for the first time of incisor morphology, an important discriminator between extant and extinct orangutan variants (see below). [What about other caves — significance test sites against each other].<sup>1</sup> Most distinctive in size is the population of Hang Hum, which dentally is extraordinarily small, and has been recognized here as representing the distinct subspecies *P.p. Hagar*. Presumed sexual dimorphism apart, none of the Vietnamese Pleistocene sites has produced any more than a single orangutan morph. It therefore remains difficult to determine with any precision at what taxonomic level such morphs should be distinguished (although it is worth repeating that the size difference separating the largest and smallest orangutan samples from Vietnam greatly exceeds the variation seen within any other primate species that we have studied).

Because the evidence of isolated teeth is far from ideal, we have preferred to be conservative here and to interpret the tiny Hang Hum variant simply as a subspecies of *P. pygmaeus*, rather than as a distinct species. We should, however, note the *P.p. Hagar* is more distinct in size from the smallest living orangutans than, say, *P.p. weidenreichi* is larger than them, and on this criterion thus the farthest outlier among all of the variants currently known. It is unfortunate that no upper central incisors are known from Hang Hum, because the major distinction in dental morphology between the two large fossil subspecies and the two smaller living ones lies in the shortness of the upper central incisor crowns and the poor definition of the lingual pillar in the former."

"In summary, the ensemble of Vietnamese Pleistocene sites reveals a much greater diversity of large-bodied hominoid taxa than previously suspected, despite the fact that only isolated teeth have yet been recovered at almost all sites. At least four genera are represented: *Pongo*, *Langsonia*, *Homo* and *Gigantopithecus*. Within *Pongo*, we are able to recognize two entirely distinctive species, *P. pygmaeus* and *P. khaei*. The latter is effectively known from only a single site in the northernmost part of the country (where it is, however, by far the most abundant hominoid), while the former is represented at virtually all sites and in a wide range of sizes. In addition to the previously described fossil subspecies *P.p. weidenreichi*, we are able to recognize at least one new subspecies in the overall assemblage, the diminutive *P.p. Hagar*. *Homo* and *Gigantopithecus* are both very rare. The former is found in at least three sites, but only at Hang Hum, where the four putative *Homo* teeth are hard to interpret (see above); it is never represented by more than one or two teeth. *Gigantopithecus* is rarer still, definitive evidence coming only from a single tooth found at Tham Khuyen. If a very badly worn tooth from the southerly site of Tham Om does not represent *Gigantopithecus blacki*, this would suggest that the historic southern limit of distribution of this hominoid broadly coincided with the modern border between China and Vietnam. Given the rarity of *Pongo p. weidenreichi* specimens from southern Chinese sites, it seems possible that the historical northern limit of orangutan distribution on the Asian mainland lay not far distant from this area. Finally, we note the existence of sparse evidence for a

small thick-enameled hominoid of uncertain affinity, and a possible additional variant of *Pongo*."

[Note: Schwartz et al. should be read after the Cuong article above in the Takazawa et al. book. Their report is both an update of Cuong and an intensification vis-à-vis orangutan varieties. Is anyone besides me impressed with how bright and droll *Pongo* is? - HF]

## MISSING LINK FOUND IN ETHIOPIA! OR?

One of worst fears of Emperor Haile Selassie was that Ferenj (< Arabic *fereng* < *Frank*, i.e., Europeans) would find a primitive human being in Ethiopia and thus use it to construct an evolutionary model which included Africans in general and Ethiopians in particular as primitive, hence savage and genetically deprived humans — in a word *Untermenschen*. He saw the concept of the “missing link” between apes and men as terribly threatening to Ethiopia, as in “what if we are the missing link?” For this reason, he was for some time very loathe to let paleoanthropologists like the Leakeys, or even deep digging archeologists, roam around his country. Once, after seeing a movie on the very tall and very naked Nuer of Ethiopia, he remarked that he hadn’t realized that “We had such people in Our country.” In other words, what a pity! We do have naked savages in Our country.

If we could wake the Emperor up, he might realize that his countrymen have changed their attitudes towards the whole topic. Nowadays, Ethiopians tend to be proud of their role as innovators in human evolution, with Ethiopia a leading candidate for the site of the Garden of Eden and Lucy as the star of the show. They love to play on her name /dɪnk’ nêʃ/ = “lovely, thou (fem) art (or) dwarf, thou (fem) art”, depending on glottalizing the /k/.

But Haile Sellasie would have to be told that his nightmare had come true! The missing link has been found in Ethiopia. Or that is what the *New York Times* announced recently (22 September 1994). In the same Awash River basin where Lucy was found, some small distance to the south at a place called Aramis, they found Lucy’s great great grand-father (so to speak). He will be called *Australopithecus ramidus* = “southern ape the root”, so-called because /ramid/ means “root” in Afar the local language. Lucy’s ancestor is 4,400,000 years old, while the calculated split between human lines and the African apes — so-called “last common ancestor” — is dated biogenetically at 4-6 mya. Lucy is circa 3.2 mya. Details about the fossils are announced in *Nature* of the same date. We have seen but not studied them. Presumably, they will justify the label *Australopithecus* instead of simply “last common ancestor”, since he lived in the ancestral period. Most telling are the data on *ramidus*’s knuckles, which indicate that he was probably not knuckle walking but rather walking upright. More information can be obtained from Tim White (Anthropology/Berkeley) or Gen Suwa at Department of Anthropology, University of Tokyo, 113 Tokyo, Japan or Berhane Asfaw at the National Museum, Addis Ababa (or P.O. Box 5717, Addis Ababa, Ethiopia).

The *New York Times* also had borrowed an evolutionary scheme from Tim White. It was quite standard, well, modern standard, in its dates and evolutionary relations. These bear brief repetition, thusly:

1) African apes and the human line split apart ca 4-6 mya.

2) Until modern times, there is no fossil evidence in the two ape lines (Gorilla and chimpanzee) for 4 or 4.5 million years! [But there are, of course, ape fossils in Vietnam (see above).]

3) *A. ramidus* begat Lucy, who begat her line *A. afarensis* ca 3 mya. The real missing link is missing between 3 mya and 2 mya when *Homo habilis* appears and leads to *H. erectus* 1+ mya.

4) Several offshoots of *A. afarensis* appear around 2.5-2.7 mya. First *A. africanus*, then *A. ethiopicus*. They either die out or develop into the next.

5) *A. ethiopicus* or his collaterals begat *A. boisei* and then *A. robustus* in the middle of the 1-2 million years ago era. There is no reason to believe that *A. boisei* sired *A. robustus*.

6) That whole line of Australopithecines in (4) and (5) died out or was perhaps bumped off by *Homo erectus*. [Why do they not propose that the Australopithecines were absorbed by *H. erectus*? Why not have a “rising tide lifts all boats” theory for that epoch?]

7) And of course, *Homo erectus* begat *Homo sapiens* in the last million years, although some say that *H. sapiens* was sired by *Homo habilis* and never had *H. erectus* for a parental species.

*Comment:* if one were to imagine the ancestor common to human beings, chimpanzees and gorillas, she would not necessarily look like or behave like a modern chimpanzee. Obviously. Yet that assumption seems embedded in the paleoanthropological thinking about the remote ancestor. I for one would be willing to call *A. ramidus* by a new name, *Homopithecus tarzanus*, proto-man-ape - HF.

### The distribution of Lucy’s tribe

It is not a trivial consideration to suppose that Lucy was an isolated northeast Ethiopian species, too limited in distribution to be ancestral to many later varieties in East Africa. The discovery of *A. afarensis* in Tanzania (Laetoli) has helped a great deal to confirm the tribe’s wide distribution. Now some new well-dated teeth in the Omo beds (extreme southwestern Ethiopia) have shown that Lucy’s tribe extended over the great Ethiopian massif and down into the southern lowlands around the lower Omo which themselves extend more or less continuously down to Tanzania. These finds also greatly increase the specific likelihood that both the southern Sudan and north Uganda will yield up some of Lucy’s kin if we all get around to digging in those places.

Moreover, the new teeth, found at Fejej near the Kenya border, are dated to >3.6 mya, possibly to 4.0+ mya, suggesting that *A. afarensis* is oldest of all around Lake Rudolf/Turkana. Of the various reports I have used “New Paleontological Discoveries from Fejej, Southern Omo, Ethiopia”. John G. Fleagle et al. 1994. However, the probable date of submission is mid-1991, since it was a conference paper published in Bahru Zawde, Richard Pankhurst, and Taddese Beyene, eds., *Proceedings of the Eleventh International Conference of Ethiopian Studies*, Addis Ababa, April 1-6 1991,

pp. 15-22.

### Summary Scheme of Ethiopian Hominids

In the same issue as the Fejej report, Tsirha Adefris, an Ethiopian paleoanthropologist, summed up the uncertainties of the potentially decisive hominid fossils found in Ethiopia. His dates are probably not too wild but not precise either, due to the nature of the dating methods and the luck of the dig. He lists the principal "stratigraphy" of greater Ethiopia for the past million years about as follows: (hominids taken from text)

Time in m.y.	Fossils	Associated hominid
<.1 (>37,000)	OmoIII	<i>H. sapiens + erectus</i>
.1	OmoI, OmoII (?)	<i>H. sapiens</i> (OmoI), <i>H. sapiens + erectus</i> (OmoII)
.2	Dire Dawa	1 Mandible; unsure
.3		
.4		
.5	BodoI, Bod VP1/1	<i>H. sapiens</i> Gradel or <i>H. erectus</i> , debate over same skull
.6		
.7		
.8	Melka Kunture	<i>H. erectus</i> ; certain

Dire Dawa is in east central Ethiopia, Melka Kunture is in central Ethiopia not far south of Addis Ababa, Bodo is in the Danakil (Afar) depression of northeast Ethiopia, while everyone knows where the Omo beds are.

OmoI has been compared to Skhul V of Israel and has at least one indication of comparable age. It lies in "Member I of the Shungura formation" which has been dated by Uranium thorium to 130,000 years ago. OmoII has been compared to *H. erectus* of Arago, Spain. Michael Day has suggested that OmoIII really belongs with OmoI.

Since Dire Dawa is associated with "early Stillbay" culture, its association with modern humans, especially Bushmen, and eastern Africa are things of fair general probability. It has cave art too. Its hominid has also been linked with "rhodesiod" [sic -HF] man and also the ill-fated Kanam jaw.

Melka Kunture is associated with Acheulean industry in the site but not necessarily produced by the resident hominid.

Most regrettably, OmoI is "associated with stone artifacts of unspecified nature and some animal bone debris (Leakey 1969)".

This year, an 11-member international team, using very high tech methods, has confirmed and added to Tsirha's scheme by concentrating new field work and analysis on the Bodo sites. They are J. D. Clark, J. de Heinzelin, K. D. Schick, W. K. Hart, T. D. White, G. WoldeGabriel, R. C. Walter, G. Suwa, B. Asfaw, E. Vrba and Y. H.-Selassie. Their article appeared in *Science* vol. 264, 24 June, 1994, p. 1907-1910,

entitled "African *Homo erectus*: Old Radiometric Ages and Young Oldowan Assemblages in the Middle Awash, Ethiopia". The abstract reads as follows:

"Fossils and artifacts recovered from the Middle Awash Valley of Ethiopia's Afar depression sample the Middle Pleistocene transition from *Homo erectus* to *Homo sapiens*. Ar/Ar ages, biostratigraphy, and tephrochronology from this area indicate that the Pleistocene Bodo hominid cranium and newer specimens are approximately 0.6 million years old. Only Oldowan chopper and flake assemblages are present in the lower stratigraphic units, but Acheulean bifacial artifacts are consistently prevalent and widespread in directly overlying deposits. This technological transition is related to a shift in sedimentary regime, supporting the hypothesis that Middle Pleistocene Oldowan assemblages represent a behavioral facies of the Acheulean industrial complex."

Besides giving the Bodo sites firm chronology, this laudable article focused more sharply on the hominid taxonomy issues and the cultural strata much more than had been done previously. As far as the bodies are concerned, they first note: "These specimens straddle the traditional morphological interface between *Homo erectus* and *Homo sapiens* — a transition whose age is poorly defined. The Bodo cranium exhibits cut marks indicating defleshing (5). This specimen is usually referred to as "archaic" *Homo sapiens*, as have other inadequately dated specimens from Europe (Arago, Petralona) and Asia (Yunxian)." [MT footnote<sup>2</sup>]

Later they say: "A distal third of a hominid humerus lacking an articular end (...) was recovered in 1990 from the surface of the upper Bodo sands... Aside from a greater cortical thickness, this fossil has no morphological features noticeably different from modern human comparative specimens. This new Bodo humerus is appreciably smaller than many modern human humeri. The two other Bodo hominid individuals are represented by large and robust cranial remains recovered from the same locality. The relatively small size of the new humerus may reflect pronounced sexual dimorphism in this hominid form, also suggested by broadly contemporary, smaller cranial remains from other African sites as Salé and Ndutu. Several workers have identified primitive characters of the Bodo crania that accord well with an older age for the fossils than previously thought (...). However, most workers have identified the Bodo specimen as "archaic" *Homo sapiens* on the basis of its derived morphology compared to typical *Homo erectus* of Java and China."

They believe their dating ("placement") of the Bodo hominids is important because it "has significant implications for the interpretation of hominid evolution across the Middle Pleistocene. Age estimates for south and east Asian representatives of *Homo erectus* overlap the Bodo age estimate (...). If these estimates for Asian fossils are correct, evolution within African and Asian *Homo erectus* may have followed different trajectories, with advanced features appearing earlier in Africa (...). Alternatively, other specimens interpreted to have evolved from *Homo erectus* (such as Arago, Petralona,

and Yunxian) may be substantially older than generally thought [.2 my earlier - HF].”

With regard to the cultures, their data clearly associate the hominids with the Acheulean. Below that there is an Oldowan industry with “simple cores and flakes typical of Oldowan technology; bifaces are noticeably absent. Flakes are predominantly cortical or plain-platformed, primarily unmodified, with rare, minimal retouch. ... In contrast, the Acheulean artifacts dominated by relatively well-made, bifacial hand axes and cleavers...” [Artifacts found in good condition - HF]

“The local shift from Oldowan to Acheulean assemblages observed in the Middle Awash comes substantially later than the appearance of the Acheulean in Africa, dated elsewhere minimally at 1.5 Mya... [Note: their “Mya” = million years ago] Oldowan and Acheulean assemblages found in contemporary Olduvai bed II deposits were attributed first to different contemporary hominid taxa (...) and then to different technological responses to different habitat (...) or raw material use (...). There is, however, a progressive change in the sedimentary regime. The wadi fan environment became increasingly dominant, and sands accounted for a greater volume of the section in subunit u3. The Middle Awash change from core (chopper)-based Oldowan assemblages to assemblages of Acheulean bifaces appears to reflect the use of different technologies in different geographic settings. Isaac (...) noted the clustering of Acheulean tools in areas with a sandy substratum, suggesting mechanisms of hominid preference and hydraulic agency to account for this observation (...). The Middle Awash data are consistent with such interpretations. Future use of the landscape approach to interpret archeological assemblages (...) will explore late Oldowan assemblages as activity facies of the Acheulian (...)”

“Older radiometric ages (0.64 Ma) for the Middle Awash and for upper Ologesailie (0.6 to 0.74Ma) combine with the discovery of much older (1.0 to 1.5 Ma) Acheulian occurrences at Konso-Gardula (...), Kesem-Kebena (...), and Bouri (28) to emphasize the long duration of the Acheulean. These findings couple with new evidence for possible early (1.6 to 1.8 Ma) hominid occupation of Java (...) to show that the Old World origins, dispersal, culture, and evolution of *Homo erectus* were very complex.”

Their footnote 28 says: “Middle Awash project work in Bouri in 1992 has yielded large collections of vertebrate (including hominid) and archeological remains that obviously antedate the Bodo assemblages on biochronological grounds, as predicted [by Clark in *Nature* 307, p. 423 (1984)].” It all sounds very exciting!

*Comment:* Their last paragraph before footnote 28 seems to have a non-sequitur in it, or they have confused me. The dates for the Acheulean in Africa need not be as old as the dates for *Homo erectus* in Java. See Bar-Yosef’s remarks above about two sets of *Homo erectus* setting out to conquer the world; the second was the carrier of Acheulean and never reached Java. The first set is associated with *Homo erectus* and Oldowan industry in Africa, unless we maintain that Oldowan was *Homo*

*habilis*’s industry exclusively. Then how did the chopper tools get to Asia, since *Homo habilis* does not seem to have left home? While Glenn Isaac’s theory may satisfy ecological reasoning preferences among archeologists, it does not sit well with what is a quite pronounced *in situ* change from Oldowan to Acheulean. Methinks, the archaic *Homo sapiens* from the south moved into the Awash basin and demographic change occurred at Bodo along with industrial change. (Unfortunately, it is impossible to tell from their report whether Bodo man is *exclusively* connected to Acheulean or not.)

## Notes

1. Editorial note: this is *not* a bracketed *Mother Tongue* comment but rather part of the original manuscript. - HF
2. Arago and Petralona of Europe are often associated with Swanscombe, Steinheim and (sometimes) Piltdown of sacred memory — all of them as archaic *Homo sapiens* or the last stage before fully modern humans. The lot of them (except Piltdown) generally are dated from .2 to .4 mya or 200-400 kya. Bräuer’s evolutionary scheme on page 405 of article (above) calls them “Ante-Neandertal” but not pre-Neandertal, meaning not necessarily parental to the Neandertals but earlier.



## TWO HYPOTHESES CLASH IN THE *American Anthropologist*

Many readers may be aware of this already, but we report on the presumption that most long rangers don't read paleontological debates in that eminent journal. The conflict was naturally about modern human origins and involved the two dominant theories which have been mentioned in *Mother Tongue* numerous times. The exchange went like this:

David W. Frayer and colleagues wrote "Theories of Modern Human Origins: The Paleontological Test" and published in the *American Anthropologist* 95:14-50 (1993). The burden of their article was an attack on "Eve theory" and a defense of "multiregional evolution" or what we have been calling the "rising tide lifts all boats" theory. They were answered by:

Chris Stringer and Günter Bräuer replied in the same journal, volume 96:416-424 (1994). In the Commentaries section, their long rebuttal was entitled: "Methods, Misreading, and Bias" and fundamentally took the position that their theory was not the same as Eve theory but rather should be called Recent African Origin Hypothesis or RAO for short. The difference was that Eve theory depended exclusively on mtDNA while RAO used that but also fossil evidence and nuclear DNA.

Herein lies our dilemma. Unquestionably, the exchange is an important matter for long rangers. HF does not want to let his own preference for RAO interfere with a fair hearing of the multiregional evolution theory (which Stringer and Bräuer call MRE), especially since one of my best friends supports MRE and is passionately opposed to Eve theory and skeptical of RAO. However, the publication of both viewpoints would consume a lot of space. And we do not know if we can get permission from the journal to do so.

So we will consider publishing the two halves, as has been suggested by one of the participants, but only after we hear from some people about the suitability of it. Please, good colleagues, tell us if you have read it already or do not want to hear about it! We'll appreciate it.

## INSIDE THE AMERICAN INDIAN LANGUAGE CLASSIFICATION DEBATE

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**1. Introduction.** The story I am about to tell chronicles a current controversy. In this personal account, I hope to clarify aspects of the current controversy concerning the classification of the American Indian languages. I present it in hopes of disarming some of the hype and acrimony, and the misunderstanding, so prevalent in the popular and semi-scientific media. That is, this is a plea to return scholarly inquiry to substantive matters.

At issue is how languages are to be classified, especially the American Indian languages. There are two principal camps. One is associated with Joseph Greenberg and his book, *Language in the Americas* (1987) (henceforth LIA). Greenberg contends that the many hundreds of Native American languages belong to only three families (Eskimo-Aleut, NaDene, and Amerind), with so-called "Amerind" containing most of these. The other approach, which most specialists favor, maintains that valid methods do not at present permit consolidation of Native American languages into fewer than about 145 independent families. The controversy over this issue shows how quixotic scholarly inquiry can sometimes become.

**2. Greenberg's procedures.** Recently, a European colleague asked me: "We've shown that Greenberg's data aren't data; we've shown that his method isn't a method; we've shown that he didn't apply his method in any case; and we've shown that even if he had, it doesn't work; what more can they ask of us?" This may seem a harsh summary, but most other American Indian linguists are in agreement with her. For that reason, it will be helpful as background to begin with the reasons for the sentiments implied in this question, before turning to the debate itself (for full support of the points mentioned here, see Campbell 1988, in press, Goddard and Campbell 1994).

What she means by "Greenberg's data are not data" is that nearly every specialist finds extensive distortions and inaccuracies in Greenberg's data: "the number of erroneous forms probably exceeds that of the correct forms" (Adelaar 1989:253; see also Campbell 1988, Chafe 1987, Goddard 1987, Golla 1988, Rankin 1992, etc.) However, for the sake of argument, let us overlook for the moment the masses of errors in the data Greenberg cites. What she means by the comment that "Greenberg's method is not a method" is the following. Greenberg assembles forms which are similar from among the languages which he compares and declares them to be evidence of common heritage. But where Greenberg stops (after having assembled the similarities) is where other historical linguists start. Since similarities can be due to a number of factors (accident, borrowing, onomatopoeia, sound symbolism, nursery

words ["mama, papa, nana, tata, caca", etc.], and universals), for a plausible proposal of remote relationship, one must attempt to eliminate all other possible explanations, leaving a shared common heritage the most likely (cf. Campbell 1988, Goddard 1975, Kaufman 1990, Matisoff 1990, Watkins 1990). Greenberg's method makes no real attempt to eliminate these other possible explanations, and the similarities he has amassed do appear to many of us to be due mostly to accident and these other factors (see Ringe 1992 for mathematical proof that chance can explain Greenberg's "evidence"). That is, Greenberg has not presented a convincing case that the similarities are due to inheritance from an earlier common ancestor.

In any case, Greenberg did not employ his highly heralded method of mass (or multilateral) comparison to establish his classification. Rather, he had already drawn his conclusions about most of the classification long before and only later began filling out his notebooks (which are available from the Stanford Library) upon which his classification is purported to rely. His arrangement/classification has not changed significantly since his 1953 and 1956 papers (Greenberg 1953, 1960), though the supporting data were assembled afterwards (Greenberg 1990:6). As is clear from the arrangement of languages in these notebooks, they were ordered to reflect a preconceived classification and "mass comparison" was not used to arrive at that grouping. Both the fact that Greenberg did not apply his method to establish his classification and that he had decided on much of it already before he assembled the data for his notebooks is confirmed by Greenberg himself:

Even cursory investigation of the celebrated "disputed" cases, such as Athabaskan-Tlingit-Haida and Algonkin-Wiyot-Yurok, indicate that these relationships are not very distant ones and, indeed, **are evident on inspection**. Even the much larger Macro-Penutian grouping seems **well within the bounds of what can be accepted without more elaborate investigation and marshaling of supporting evidence**. (Greenberg 1953:283.) (My emphasis, LC.)<sup>1</sup>

As a result of this preconceived classification, some language groupings in LIA (which follows extensively earlier proposals by Edward Sapir [cf. Sapir 1929] for North America and Paul Rivet [cf. Rivet and Loukotka 1952] for South America) are now known to be indisputably wrong, and there is no way these parts of Greenberg's classification could have followed from an application of mass comparison (or any other method) to the data. For illustration's sake, I cite just one erroneous classification from each of these two scholars which Greenberg incorporated into his classification. Following Rivet, Greenberg classified Uru-Chipaya and Puquina as closely related languages, when they have almost nothing in common, based on the old misunderstanding derived from the fact that Uru-Chipaya is commonly called Puquina by many outsiders (Adelaar 1989:252, Olson 1964:314).<sup>2</sup> Following

Sapir, Greenberg placed Subtiaba-Tlapanec with Hokan, now known to be a clear and undisputed branch of Otomanguean (cf. Campbell 1988, Suárez 1983, 1986). The data from these languages do not lead to Greenberg's classifications of these languages. (For other such problems with the classification, see Adelaar 1989, Campbell 1988.)

My European colleague's last point was that Greenberg's method does not work. That is, it is incapable of distinguishing American Indian languages from others selected randomly. With Greenberg's method and the examples he presented as evidence for Amerind, it is easy to show that such non-American languages as Finnish (or Finno-Ugric, or Uralic), Basque, and others fit into his Amerind group just as well as most of the American Indian languages which he has claimed to be its members (for a full demonstration of this point, see Campbell 1988, in press).<sup>3</sup>

Indeed, what more can be asked of us?

One of the most telling aspects of the whole debate is that most American Indian linguistic scholars are not opposed to distant genetic relationships, but in fact Greenberg shares our research objectives of working towards reducing the ultimate number of linguistic groupings and of finding more remote family relationships among the American Indian languages. Most American Indian linguists believe it possible, perhaps even probable, that most American Indian languages are genetically related. The main difference is that we find Greenberg's methods and evidence inadequate. In short, when people who are already predisposed towards something (— in this case towards the reduction of linguistic diversity in the Americas and the possibility that the languages are related —) have trouble accepting it (— this case being Greenberg's attempted reduction —), there is probably good reason for the caution.<sup>4</sup>

Most other American Indian linguists, in contrast to Greenberg, hold that given (1) the limitations of valid methods, (2) the vast amount of linguistic change these languages have undergone since they began to diversify, (3) and that we do not know how many different groups or languages entered the New World, we cannot at present reduce the number of independent linguistic families in the Americas to fewer than about 145. Hopefully as the work progresses, new relationships will be established, and this number will become smaller. However, we must also face the possibility that these limitations may be so great that we may never be able to determine the picture fully.

In sum, when 80% to 90% (in Greenberg's words [quoted in Lewin 1988:1632]) of specialists in this field, who share Greenberg's goals, reject his proposals, there is a good chance that there is something wrong with Greenberg's conclusions, much publicized though they may be.

**3. The debate.** The way I became involved in the controversy reveals something about the nature of the debate, and I therefore relate some of the story in hopes of clarifying aspects of the dispute.<sup>5</sup>

My involvement in the classification of American Indian languages became more public as a result of the

Linguistic Institute, summer 1976, at Oswego, N.Y., which had Native American linguistics as one of its foci.<sup>6</sup> I was to obtain scholarship funds to support study by Native American students at the Institute. As it turned out, funding agencies did not provide such support; however, in support of the Institute's focus, I submitted an application for a conference grant to the National Science Foundation which was funded and the papers from this conference became *The languages of Native America: an historical and comparative assessment* (Campbell and Mithun 1979b) — henceforth LNA, a book which has turned out to be much cited.

Greenberg was also at that 1976 Linguistics Institute, and I asked him about his views, having heard of his forthcoming book on the classification of the American Indian languages. However, he did not tell me (I do not know why). He did not attend the conference to reassess American Indian historical linguistics (from whence LNA), whose participants were many of the foremost scholars in this field (although Greenberg was present at the Institute).

The introduction to LNA was to have been written by another conference participant, but other obligations later made it impossible for him to write it. Therefore, it was written hastily by Marianne Mithun and myself to meet last-minute deadlines. This introduction, together with the other contents of the book, turned out to receive considerable attention, and as a result I became publicly associated with American Indian linguistic issues through the twin accidents of no available funding for Native American scholarships (hence the conference grant that led to LNA) and circumstances which led to the writing of the introduction.

**3.1. Lumpers versus Splitters.** The simple message of the Oswego Conference and LNA was that many of the widely accepted distant genetic proposals, often originally advanced as hunches or long-shots, had never been tested adequately. LNA called for a reassessment of the evidence and for the application of reliable methods in the investigation of these far-flung proposals. Given the great number of distinct Native American languages, scholars in the past set out to reduce the vast linguistic diversity to manageable genetic schemes. The history of American Indian linguistics was characterized by rough-and-ready hypotheses of possible family connections, which lumped languages into ever larger groups with the intent of reducing the ultimate number of independent genetic units in the Americas. Often these were offered as very preliminary proposals to be tested in subsequent work, but unfortunately many of these came to be accepted uncritically and were repeated in the literature until they were so entrenched that many believed they had been established through valid procedures. This acceptance of the far-flung yet undemonstrated hypotheses of distant genetic relationships was abetted by the faith American anthropologists and linguists had in the intellectual abilities of Sapir and Kroeber, very influential lumper scholars. (Cf. Campbell and Mithun 1979a, Darnell 1990, Golla 1984, for details.)

The upshot was that such early proposals as Penutian and Hokan (first proposed for California languages) attracted

extensive attention, and more and more as yet unaffiliated languages were suggested as relatives of one or the other of these proposed broad groupings. Soon there were proposals which attempted to link languages from Alaska to South America as members of Penutian or of Hokan.

After LNA, the so-called “splitter” orientation, i.e. the call for more careful scrutiny and closer assessment of the evidence, became more dominant and today most specialists support such a view.

**3.2. Posturing, “Shouting Down”, and Rhetorical Plays.** Eventually, Greenberg's claims began to appear before a broader audience. *Current Anthropology* asked me for comments on the Greenberg, Turner, and Zegura (1986) manuscript, comments to be limited to about 1/2 column. Given Greenberg's deserved reputation in other areas, it seemed clear to me that this article would confuse non-specialists, and so I tried to offer a warning (Campbell 1986). Greenberg has repeatedly cited my comments in 1986 in his claim that I dismissed his classification before it was published (LIA appeared in 1987) and that therefore I could have no understanding of it. However, Greenberg's position on the classification of American Indian languages was well known from his earlier publications (Greenberg 1953, 1960, 1962, 1979, 1981b) and from recordings of his public lectures on the topic; similarly, I was well aware of his methods from both his methodological publications and his discussion of methods in his other classifications (cf. Greenberg 1955, 1957, 1963, 1969, 1971, 1981a); furthermore, I had discussed his methods in print (Campbell 1973, 1978). I knew the Greenberg et al. (1986) article was going to lead to confusion, but how much balance could my response possibly achieve in the limited space allotted? I reasoned that I should attempt to point out as best I could in the space available the major flaws in the authors' basic arguments (e.g., to name just one, that the so-called “Na-Dene” [called earlier “greater Northwest Coast”] dental group does not correspond to the proposed Na-Dene language grouping, but rather has representatives from all three of Greenberg's linguistic families), and then to leave a strong caution so that readers would at least be aware that Greenberg's language classification is not generally accepted by specialists in the field. Thus, in this context, I wrote:

Conclusion: neither their linguistic classification nor its dental/genetic correlation is supported, the conclusions about migrations are unwarranted, and the whole speculative venture should be abandoned. Indeed, the linguistic classification should be shouted down in order not to confuse nonspecialists or detract from the real contributions linguistics can make to prehistory. (Campbell 1986:488.)

It is easy to see with the benefit of hindsight that I chose my words poorly. I meant that knowledgeable scholars should “speak up” to show the flaws in Greenberg's classification, not that his hypotheses should have no hearing at all. This has been cited repeatedly (by Greenberg, and by

members of the popular press, out of context) as evidence, so they assert, of outrage, or of such a closed mind that I would reject Greenberg's proposals (so they say) before it was even published. However, Greenberg attempted in LIA to establish a posture whereby any criticism would be seen not as legitimate scholarly dialogue, but as the emotional laments of a recalcitrant establishment:

what is attempted in this work [LIA] runs against the current trends in Amerindian work and will be received... with something akin to outrage. (Greenberg 1987:43)

My conclusion that Greenberg's classification and the unsubstantiated claims of dental and genetic support should be shouted down in order not to confuse non-specialists was held up as evidence of precisely the "outrage" that Greenberg appears to have been waiting to call upon in order to forestall normal scholarly criticism, or, in his words:

I believe that readers of *Language* should be made aware that one year before my book appeared in print ... Campbell [1986] wrote that my classification of American Indian languages "should be shouted down". Under these circumstances, **an objective review could hardly be expected** [emphasis mine, LC]. (Greenberg 1989:107.)

It should not be forgotten, however, that my conclusion was scarcely harsher and certainly no more negative than most of the other comments — particularly those by nonlinguists — published with the Greenberg et al. (1986) article.

I became even more deeply involved in the controversy through the turn of events which resulted in my writing a review article (Campbell 1988) of LIA.<sup>7</sup> I was reluctant, because I did not want to spend my time in this way, addressing claims which I felt were so ill-founded as not to merit much attention. Still, I recognized the importance of peer evaluation and of attempting to counterbalance Greenberg's claims, since non-specialists were being misled. Since I had been asked to respond, I obliged, trying to argue issues of content and method, i.e., matters of substance. The result: vilification. I recount a few instances to illustrate what I mean here, beginning with the Stanford Conference.

During the summer Linguistics Institute of 1987, a conference was held at Stanford University (funded by NSF) on linguistic change and reconstruction methodology in which leading specialists in many language families and areas participated (see Baldi 1990); Ives Goddard and I were the coordinators of the American Indian linguistic section (see Campbell and Goddard 1990). An account of this conference was offered by Harold Fleming (1987a:24) in an issue of *Mother Tongue*:

It is an "ambush" when one blunders into a situation through ignorance or whatever and one gets attacked. Of course, there is no ambush without someone setting

it up secretly. If one is caught in an ambush and one's group is slaughtered, then one uses the word "massacre". So it was at Stanford ... The Americanists had an ambush in mind. They came to attack Greenberg and the other Lumpers ... I regret that this is not a florid or inaccurate description of five days on that lovely campus. It is an ethnographic conclusion, from participant observation ... the Amerind Border Patrol, especially Campbell, Goddard and Mithun ... attacked very aggressively ... Campbell's attacks on Greenberg became personal and vile. For example, I heard a quote something like this, "Greenberg is lucky that he had Stanford University Press to publish his Amerind book because no one else would have touched it!" ... He's [Greenberg] not a conference brawler and does not like confrontations ... Rare indeed is the scholar who will argue publicly with a loud, aggressive expert, especially on the expert's own turf!

The actual events of the conference (that is, my version of them) differ totally. As organizers of the American Indian linguistic section, Goddard and I purposefully placed the discussion of distant genetic relationships (where Greenberg's proposal properly belonged) in the last hour of the nine hours of workshop discussion. We would have preferred not to discuss the matter at all, since Greenberg's treatment of distant genetic relationship involves no reconstruction, hence no reconstruction methodology, which was the theme of the conference. I felt like someone entering the lion's den, on Greenberg's home turf — certainly not my own — at Stanford. What Fleming took as an example of a "personal and vile" attack was my reference to the fact that LIA neglected virtually all the published work in American Indian historical linguistics for the last twenty years (as seen, for example, in their absence from Greenberg's bibliography; cf. Campbell 1988:592). Given ordinary canons of scholarship, someone with less stature than Greenberg might have found it difficult to get a book published which neglects the primary work of a whole discipline for such a long period of time. My attempt here was not to take cheap shots at Greenberg, but to point out that some genetic groupings among American Indian languages had been demonstrated which are misassigned in LIA, while other earlier proposals now demonstrated to be erroneous are maintained in LIA — had the literature of the last two decades not been neglected, some of these absolute errors would probably not have found their way so easily into Greenberg's book.

From my perspective, then, a more inaccurate and misperceived account than Fleming's of what happened at the Stanford conference is scarcely imaginable. Nevertheless, Fleming's version illustrates how honest, if reluctant, attempts at substantive scholarly evaluation of Greenberg's work have been greeted in some quarters.

Unfortunately, rhetoric such as Fleming's is not uncommon in this controversy. This rhetorical response has been triggered solely by legitimate scholarly criticism of LIA; this criticism has erroneously been labeled — without

documentation — as personal and ad hominem attacks; the rhetoric includes unfounded attacks on me and others with similar views. The critical discussions of LIA are not received as normal scholarly dialogue, as peer evaluation, but rather have routinely been ridiculed or caricatured as “shrill attacks” (Ruhlen in press b, and “vehement disputes” (Ross 1991 [staff writer for *Scientific American*]); other terms one sees, are “strident outcries, anger, vituperation, vehemence, outrage, aggression”, etc. (cf. Lewin 1988 [a writer for *Science*], Ross 1991). Thus one reads that “[Campbell] rejects angrily” (Greenberg 1990), “Campbell ... express[es] outrage” (Bower 1990 [reporter for *Science*]), “[Campbell] rejects Greenberg’s position with the vehemence of a litigator” (Ross 1991), and so on. Greenberg’s critics in general are accused of “added dose[s] of *ad hominem* invective” (Ruhlen in press a), and, “the vituperative attack on the Amerind phylum by [Greenberg’s critics] reflects their **blind prejudice and basic ignorance**” (Ruhlen in press a [emphasis added, LC]).<sup>8</sup>

What can one do to set the record straight in the face of such rhetorical responses which amount to character assassination, where I and other like-minded scholars are flatly branded with epithets declaring us to be “blindly prejudiced”, “ignorant”, “aggressive”, “lacking objectivity”, “vehement”, and “vituperative”? A deconstructionist might well invert the interpretation here, finding that **the labels Greenberg and his few followers apply to their critics (including me) are more accurately seen to be descriptions of the actions which the labelers themselves have engaged in**, the real source of the “vehemence” and “vituperation”. There is an element of African *déjà vu* in the tough rhetoric emanating from the Greenberg side of the current controversy:

This work [Greenberg 1955] was much less revolutionary than was thought at the time (especially in the United States), except perhaps for the **violence with which the author [Greenberg] attacked both his predecessors and contemporaries ...** One might particularly criticize this work because of **its tone, a tone which gained it a *succès de scandale*** rather than a scientific success. (Emphasis added, LC.) (Alexandre 1972:69.)<sup>9</sup>

Greenberg’s (1990, Newman 1991) most recent statements illustrate the problems of rhetoric which characterize the controversy. A few points from these statements suffice to document this.

(1) Greenberg has often said in public lectures, but in Greenberg (1990:12) for the first time in print I think, that American Indian linguists are basically ignorant of their own field, with little knowledge of different American Indian language families and of others in the world. He writes:

All the science writers emphasize that a large majority of American Indianist linguistic scholars disagree with me. These writers are simply taking it for granted they [the large majority of American Indianist linguists] all have a wide acquaintance with the primary data and

have had experience with and made contributions to classifications ... this does not hold true. (Greenberg 1990:12.)

And again:

But what they [anthropological linguists working with American Indian groups or African languages] lack, let’s say, in regard to American Indian languages, for example, is a broad view of what languages are like — even what American Indian languages are like. (In Newman 1991:461.)

This is false. A large number of American Indianist linguists could be cited as counterexamples, who have extensive experience and both have contributed to language families in the Americas and have expertise in other languages as well (e.g. Howard Berman, William Bright, Wallace Chafe, Adolfo Constenla, Ives Goddard, Victor Golla, Mary Haas, Kenneth Hale, Eric Hamp, Jeffrey Heath, William Jacobsen, Terrence Kaufman [mentioned by Greenberg as a possible exception], Michael Krauss, Robert Longacre, Floyd Lounsbury, Norman McQuown, Marianne Mithun, Robert Rankin, Richard Rhodes, Michael Silverstein, and Allan Taylor, to name but a few). I dare say that my Americanist colleagues and I have far, far more direct hands-on experience with American Indian languages, their “primary data”, their structure and their linguistic changes, and questions of their classification, than Joseph Greenberg. He has never worked with an American Indian informant (Newman 1991:457), never learned to read or speak any American Indian language, or done any significant descriptive work on any Native American tongue! It is he who has no obvious in-depth knowledge of any American Indian language or family, with only selected secondary knowledge derived from his seeking limited words and morphemes from published sources in order to fill in the blanks of his preconceived notebooks. Yes, he has scuffled with the sources (mostly older ones), but not in a way to give him the privileged position he is adjudicating to himself and negating to others.

But all this is beside the point. Most American Indian linguists read each other’s work, listen to each other’s papers at professional meetings, and in general participate in a network of shared information and ideas — they are on the whole well informed about their field and how developments in linguistic theory and historical linguistics may relate to it; that is as it should be.<sup>10</sup> Wide acquaintance with the primary data (even the limited sort sought out by Greenberg for his notebooks) is not the issue, but rather what is done with the sources available and whether the proposed classifications hold up, irrespective of the erudition or lack thereof of the researcher. The vast majority of specialists have weighed Greenberg’s evidence and rejected his classification precisely because they find the case he presents unconvincing. This, in my opinion, is evidence not of their lack of erudition, but rather of their having exercised the high degree of sophistication which in general characterizes this field.

(2) Greenberg (1990:6) declared that Lamb (1959),



Swadesh (1960), and he (Greenberg 1960[1954]) had independently arrived at the "fundamental threefold classification for the Americas" at roughly the same time, and that:

the fact that three scholars arrived at the identical conclusion within the same period of roughly four years, 1956-1960, argues strongly for its inherent likelihood.

First, the hypothesis of a three-way classification of American Indian languages was well-known long before Lamb, Swadesh, and Greenberg; they did not arrive at this notion independently in the 1956-1960 period, but rather merely repeated a thesis prevalent in the American Indian linguistic tradition at least since Sapir's early writings. Greenberg's tripartite classification (i.e. Eskimo-Aleut, Na-Dene, Amerind) is not new. Sapir presented a three-way classification on several occasions (Sapir 1916, 1990[1920]:86, 1921, 1929; cf. Darnell 1990:123, Golla 1984:452).<sup>11</sup> To quote Ruhlen (1987:222):

Not only was Sapir aware of the apparent existence of an enormously widespread American stock by 1918, but even earlier he had remarked on the fact that Eskimo-Aleut and Na-Dene seemed to stand apart from the other New World languages.

Greenberg's three groupings clearly continue an established tradition (cf. Lamb 1959:36; Swadesh 1952, 1954:307, 1960:896), followed by others. This three-group view had also diffused widely into the thinking of non-linguists on the matter (e.g., Bray 1986, Carlson 1983:96, Williams et al. 1985, to mention just a few ready examples). Moreover, Swadesh, Lamb, and Greenberg did not arrive at their classifications independently of one another, either. These three scholars were in contact, and Greenberg worked extensively with Swadesh on a number of classification projects.<sup>12</sup>

Second, Greenberg was well aware of these facts — for example Campbell (in press, which Greenberg 1990 cites), charts this history of the tripartite classification. More to the point, it is common knowledge; for example, it is clearly stated by Ruhlen (1987:222-30), though in a way favorable to Greenberg. Not to be aware of this would be tantamount to poor scholarship beyond belief.

Third, Greenberg's is a faulty argument. The fact that these three persons agree does not mean that the majority of scholars agrees. Many others also working on this question do not believe that the evidence supports Greenberg's claims. Greenberg himself admits, as mentioned above, that 80% to 90% of American Indian linguists support the approach of LNA, rejecting his classification (quoted in Lewin 1988:1632). This would argue more strongly for LIA's inherent unlikelihood than would the three votes Greenberg counts in his favor.

Other rhetorical claims in the Greenberg 1990 paper have a similar character.

(3) Greenberg (1990:11-12) states that "at the Boulder

Conference, the correlation [of his classification] with the dental evidence held up completely."<sup>13</sup> Yet serious reservations were raised (as reported, for example, by Morell [1990:440-1] [*Science* staff writer]). Among various challenges, it was pointed out that Turner's dentition sample contains very few old teeth (from pre-European contact) and that the so-called Na-Dene dental group, pivotal in the argument, in fact does not correspond well with so-called Na-Dene languages, but rather contains representatives of all three of Greenberg's linguistic groups (cf. Campbell 1986, in press, Laughlin 1986:490, Szathmary 1986:490). Similarly, Greenberg cites support from Cavalli-Sforza et al.'s (1988, 1989) claims about population genetics (again very controversial; see Bateman et al. 1990a, 1990b, Morell 1990, Nichols 1990, O'Grady et al. 1989, for criticism), and from Wallace's mitochondrial DNA claims. However, Rebecca Cann's report at the Boulder Conference of at least 11 and probably more like 33 American "Eves" (representing multiple migrations or migrations with a very large gene pool in them) was not mentioned by Greenberg (cf. Morell 1990). The most generous thing one can legitimately say about the hypothesized dental and genetic correlations is that they are controversial at best (for more detail, see Laughlin 1986, Szathmary 1986, and other discussion of Greenberg et al. 1986).

More to the point, potential correlations with non-linguistic evidence (from dentition, human genetics, and archaeology) are ultimately irrelevant to issues of remote linguistic affinities, as shown by Greenberg (1957, 1963) himself. As indicated by Newman (1991:454), there is an irony in Greenberg's appeal to non-linguistic evidence in support of his American Indian linguistic classification, since Greenberg (1963) demonstrated that external non-linguistic evidence is irrelevant and often misleading in linguistic classifications. Thus, Greenberg reaffirmed the independence of race, culture, and language, and that only linguistic evidence is justified in considerations of linguistic genetic relationship and classification (cf. Newman 1991:456, 459).

This is not to discount non-linguistic evidence in the search for the history of the earliest Americans. Given that many different possible scenarios for the peopling of the Americas are consistent with the limited vision we can get currently from the linguistic record, it may turn out that archaeological and human biological information will prove far more revealing than linguistics in discovering the past of the first Americans. (Cf. Campbell in press, Goddard and Campbell 1994.)

(4) Greenberg claims there is a widespread pronoun pattern in the Americas of *n* "first person" and *m* "second person" (a pattern long known in American Indian linguistics, cf. Sapir 1915, Michelson 1914, 1915, Campbell in press) and that this is particularly strong support for his classification (cf. Greenberg 1990:11). I showed (Campbell in press) through a number of arguments that Greenberg's claims regarding the pronouns do not hold up (cf. also Goddard and Campbell 1994). Greenberg's (1990:11) only response to this battery of negative evidence was the singling out of one minor case for ridicule:

In a remark at the Boulder Conference Campbell attributed such a preponderance of nasals [in the pronouns] to the phonetic nature of infant sucking reflexes!

Greenberg has repeated this misrepresentation in a variety of interviews and papers; he implies this is an unreasonable hypothesis without stating why. However, his "infant sucking reflexes" is not an accurate account of what was said. What he alludes to appears in a list of explanations (some my own, many from other scholars) which have been offered to explain the putative *n/m* pronoun pattern — this one comes originally from Ives Goddard (1986:202), where the matter of nursing is only a part of the story, where Greenberg's is a garbled caricature of it. The text he is citing actually reads (Campbell in press):

Another explanation [not my own in this case, rather originally from Ives Goddard] that has been offered is child language; child-language expressions around the world abound in self-directed and other-directed words containing nasal consonants ... [It] is [a] universal physical fact that a gesture equivalent to that used to articulate the sound *n* is the single most important voluntary muscular activity of a nursing infant. As Goddard (1986:202) points out, possibly this factor and the tendency for primary grammatical morphemes to consist of a single, unmarked (phonetically commonplace) segment account for the widespread appearance of *n-* in "first-person" pronouns. Incidentally, in many societies, particularly among hunting and gathering groups, infants may continue to nurse until the age of five, sometimes longer, well into and beyond the age of language-acquisition.

Greenberg failed to mention the other more relevant and damaging facts which I reported, such as (1) the *n/m* pattern is not nearly as common in the Americas as Greenberg claimed, (2) it is also found fairly frequently in other languages scattered around the world, (3) his supposed *m/t* pattern for his Eurasiatic languages is also found abundantly in the Americas (despite his and Ruhlen's assertions to the contrary), and (4) a whole series of facts explain why nasals (the perceptually most salient sounds) tend to show up in forms for pronouns, etc. (There is not space here to repeat the full series of arguments and their documentation, but see Campbell in press, Goddard and Campbell 1994.) Greenberg's dismissal of this extremely damaging evidence with but a distortion of "infant sucking noises" is not sufficient.<sup>14</sup>

(5) Finally, Greenberg repeats again and again that I (and other like-minded American Indian specialists) insist on sound correspondences as the only legitimate source of evidence for genetic relationship (cf. Greenberg 1990:8-9, 1991:127-9, in press). Clearly I (and most others) do differ from Greenberg with respect to what we consider acceptable methods; however, in the matter of sound correspondences, Greenberg consistently misrepresents us. I have, quite to the

contrary, consistently insisted that sound correspondences are neither sufficient (since borrowings can also exhibit corresponding sounds) nor necessary (since patterned grammatical evidence may be sufficient in some cases) for the establishment of the plausibility of a proposal of distant genetic relationship. Certain sorts of patterned grammatical evidence (that which resists explanation from borrowing, accident, or typology and universals, especially that called "submerged" in the literature [cf. Campbell 1973, 1988]) can be important testimony, independent of the issue of sound correspondences (though I am clearly much fonder of sound correspondences as one strong and important source of evidence than Greenberg is) (see Campbell 1973, 1978, Campbell and Kaufman 1980, 1983, Campbell and Mithun 1979a, Migliazza and Campbell 1988; Campbell in press — cited by Greenberg — has a section explicitly devoted to this).

How is one to interpret this mistaken representation of my and others' position? Could this be Greenberg's attempt to simplify matters in order to contrast his methods more fully with those of American Indian linguistic specialists? This would hardly be necessary, since most American Indian linguists readily admit — I insist on — the great difference between our methods and his. Perhaps this misrepresentation is an attempt to make our methods seem simplistic and thus less reliable than his in comparison.

**4. Back to Africa.** Greenberg asserts repeatedly that his success in classifying African languages makes it likely that his American Indian classification will be correct. However, given that the assumed African success is limited, appeal to it would appear to be but another rhetorical ploy. For this reason, clarification is important. In attempting to understand the African classification, I have been impressed by the fact that American Africanists<sup>15</sup> can express their loyalty to Greenberg's classification while at the same time recounting severe problems with it. That is, most believe Greenberg's classification was a monumental success, yet they also agree that, for example, Khoisan is probably wrong and that Nilo-Saharan, as Greenberg's catch-all phylum, is so far-flung it could never possibly be demonstrated (see below). One wonders how a track record where two out of the African big four groups are dismissed results in the continued effulgent declarations of success for the classification? Thus, it is important to ask what kind of success it is?

As has been pointed out frequently, much of Greenberg's African classification simply repeats the correct classifications of earlier scholars (cf. Gregersen 1977, Welmers 1973). Likewise, as pointed out above, much of his American Indian classification repeats the earlier proposals of Sapir for North America and Rivet for South America. A difference, however, is that many of these repeated American proposals have not received acceptance and remain controversial, while some others have been shown completely wrong (see above). Thus, the part of Greenberg's strategy that helped to secure a measure of success for his African classification — that of repeating earlier accurate proposals — has, ironically, contributed to lack of success for his American proposals.

Others of Greenberg's African hypotheses involve such depth and internal diversity that they remain unproven and probably can never be demonstrated (Bender 1987, Bender et al. 1976, Welmers 1973:16-19; cf. Hymes 1959:53).

It is important to inject some realism in the discussion, and therefore those aspects of Greenberg's African alignments which are considered mistaken and others where the proposals are yet undetermined (undemonstrable?) should not be neglected:

His [Greenberg's] approach [in his African classification] is largely inadequate for the PROOF of genetic relationship; it can do little more than offer initial hypotheses, to be substantiated by more reliable techniques like the comparative method. In a number of instances, languages or language groups have been placed in a given family solely on the basis of a handful of "look-alikes" ... The Nilo-Saharan family, in particular, must be regarded as a tentative grouping, the genetic unity of which remains to be established. (Heine 1972:32.)

I would suggest that parts of Greenberg's famous classification of African languages, which was posited on the basis of multilateral comparison and more or less achieved the status of orthodoxy ..., urgently need to be reinvestigated by reliable methods. (Ringe 1993:104.)

Fleming's (1987b) description of Greenberg's African procedures and his assessment of the outcome is very telling in this regard, particularly so, since Fleming is a recognized Africanist and a well-known enthusiast of proposals of long-range relationships:

It [Nilo-Saharan] has also been called "Greenberg's waste basket," hence a collection of hard-to-classify languages and a very unreliable entity as a phylum. Vis-a-vis AA [Afro-Asiatic] or N-K [Niger-Kordofanian], N-S [Nilo-Saharan] is widely viewed as the more shaky of the three, but it no longer gets the kind of stubborn opposition that Khoisan receives in South Africa and Britain. When Greenberg finished his first classificatory sweep of Africa, he ended up with fourteen phyla. Of those, one was AA. One was N-C [Niger-Congo], which then had Kordofanian joined to it. The fourth was Khoisan. All the rest, or 10 phyla of the first classification, were put together as Nilo-Saharan. It represents far less consensus, far less agreement on sub-grouping, and very little progress on reconstruction. (Fleming 1987b:168-9; cf. also Bender 1991, 1993.)

That is, as Fleming (basically a strong supporter of Greenberg) indicates, two of Greenberg's four African groups, Khoisan and Nilo-Saharan, are widely contested. M. Lionel Bender, also sympathetic to Greenberg's classification and also a well-

known specialist in African languages, expresses similar judgements: "controversies remain in the case of all four phyla established by Greenberg" (Bender 1989:1).<sup>16</sup>

In short, acknowledging Greenberg's African success — realistically — does not deny his American fallibility. It is now possible to speak again of the African classification's methodological short-comings due to its reliance on superficial lexical similarities:

Although Greenberg's work represents considerable progress over that of previous writers, it leaves a number of questions open. His approach is largely inadequate for the PROOF of genetic relationship; it can do little more than offer initial hypotheses, to be substantiated by more reliable techniques like the comparative method. In a number of instances, languages or language groups have been placed in a given family solely on the basis of a handful of "look-alikes", i.e. morphemes of similar sound shape and meaning. The Nilo-Saharan family, in particular, must be regarded as a tentative grouping, the genetic unity of which remains to be established. (Heine 1992:31-6.)

But more importantly, if Greenberg is going to call upon his African innings as indicative of probable success in his American classification, then his batting average can be tallied only after all his times-at-bat have been factored in. Since his Indo-Pacific hypothesis (which would lump "the bulk of the non-Austronesian languages of Oceania" from the Andaman Islands [Bay of Bengal] to Tasmania, Greenberg 1971:807) has no supporters among specialists — a total strike out —,<sup>17</sup> since his African classification has to be qualified (— a base hit or two, certainly no home-run —), and since there are, regardless of other disputes, a number of absolute and uncontested errors in the classification of a number of American Indian languages, Greenberg's batting average is not a telling argument in his favor by any means.<sup>18</sup> Whether any aspect of his American classification holds up is totally independent of his work in Africa and elsewhere, irrespective of its accuracy or lack thereof. This is an empirical issue, and rhetorical posturing for America from an African platform is irrelevant.

**5. Conclusions.** While I suspect that my plea to all involved in the issue to abandon rhetoric and to return to matters of substance will be to little avail (recalling how this backfired in 1986 in my comments in *Current Anthropology*), I nevertheless hope that the account of the controversy I have presented here will have some salutary effect and that it has clarified the nature of the conflict. There can be no reason for such debate to descend to ad hominem attacks. My own conclusions regarding Greenberg's classification have been strongly negative, but they are based on purely scientific considerations. Personal and gratuitous attacks can only reflect negatively on our profession, and in any case it just will not do to try to hide issues of substance under rhetorical debris. Moreover, since the Greenberg hypothesis has been rejected in peer review in the

scientific linguistic literature, it appears that he and his scant few followers are attempting to achieve through strong rhetoric in the popular press and semi-scientific media what substantive arguments in scholarly outlets could not — an end-run around the normal checks and balances on scholarship. When substance rather than rhetoric once again takes center stage, this will no longer be possible.

### Notes

1. Notice, incidentally, that if these relationships really were as obvious as Greenberg asserts them to be, then there would be no dispute; they would be accepted. However, the Athabaskan-Tlingit-Haida (or Na-Dene) classification remains just as controversial, perhaps more so, today as then. This is precisely because it is not obvious and methods such as those employed by Greenberg cannot resolve the issue.

2. The error was pointed out and the differences between Puquina and Uru clearly shown long ago. In fact, there are many variant names for Uru: Uro, Huro, Ochomazo, Ochozuma, Uchumi, Kjotsuni, Bukina, Pukina, Puquina, Urocolla, Uroquilla, and Yuracare (not the Yuracare of eastern Bolivia).

3. I ignore here the question Greenberg raises (Newman 1991:454) of whether Greenberg applied his own method correctly.

4. Hymes (1971:264) raises a question about the reception of Greenberg 1960, which I now ask about Greenberg 1987:

Without detracting from the merit of Greenberg's work, it is historically revealing to compare the reception of the classification of South American languages by the two men [Greenberg and Swadesh]. The two classifications agree on the essential unity of the languages of the New World, differing on various internal groupings. Greenberg's classification was obtained with a list of 30 to 40 glosses, Swadesh's with a list of 100 glosses ... Greenberg published the result without supporting data, backed essentially only by personal authority. Swadesh presented an explicit account of his procedures, endeavored to make the data available, and regularly revised his findings in the light of new evidence and research. The classification based on authority without supporting evidence has been reprinted often in anthropological textbooks and journals; the work presented as an explicit, continuing scientific enterprise has not.

Swadesh's work was very similar to Greenberg's in many ways, in its conclusions, its data, and the general methods employed. Why, then, was Swadesh, extremely erudite in his first-hand knowledge of American Indian languages, rejected or ignored, while Greenberg's classification is frequently repeated with approval in the media and in other fields, although it is

denied almost wholesale by American Indian linguists? The reason, whatever it may be, does not have to do with the quality of the evidence or methods Greenberg offers, since it is precisely these which specialists find unconvincing (as they did ultimately also with Swadesh's).

That is, factors other than the scientific legitimacy of the case presented are at play in the reception of Greenberg by non-linguists. This was also true in the case of his African classification; in Greenberg's own words:

... at the beginning external [non-linguistic] things had much to do with acceptance of the African classification. All in all, I think that these external factors have had a greater impact than the arguments about linguistic methodology. (Newman 1991:454.)

This is even more the case with respect to his American Indian linguistic classification, almost universally rejected by specialists, but nevertheless given much attention by others.

5. I feel it important to point out that I cite aspects of my own involvement in this debate very reluctantly and only because I have been connected with the controversy; this involvement illustrates the matters under discussion here. I wish it were otherwise.

6. Perhaps, since this is a personal account of the debate and because Greenberg has made language experience an issue (see later in this paper), I should mention some of my earlier language involvement. I worked on Finno-Ugric, Mayan, Nootka, Uto-Aztec, and Quechua, with a dissertation on the K'ichean subgroup of Mayan (Campbell 1971). Later, I had several grants to investigate languages in Central America and southern Mexico which were nearing extinction. This involved me in historical linguistic research with languages from several additional families (Lencan, Xincan, Mixe-Zoquean, Otomanguean, Jicaquean, Misumalpan, and Chibchan), and got me involved with the issues of their linguistic affiliations and the overall picture of American Indian language classification which arose naturally in this research.

7. The former editor of *Language* had suggested I be asked to review LIA, though Terrence Kaufman had understood from the succeeding editor that he would write the review. Given this complication, for a while Kaufman and I thought that we would write the review jointly. In the end, however, Kaufman was not involved because he already had things in forthcoming issues of *Language* and the new editor, in Kaufman's same department at the University of Pittsburgh, wanted to avoid favoritism and even the appearance of favoritism. Thus, I wrote the review alone.

8. I hasten to point out that, while I have cited several negative comments here, they come from a very small group of people. They are limited largely to the writings of Greenberg and Ruhlen, and to a very few mostly discounted boosters of even more remote genetic relationships (Fleming, Shevoroshkin),

and to magazine articles which report interviews with these scholars.

For example, Shevoroshkin [linguist associated with claims for "Proto-World" and with other proposals of far-flung remote relationships] (quoted in *Atlantic Monthly*, Wright 1991:40 [free-lance writer]) says "their [Lyle Campbell's and Ives Goddard's] brains are so twisted," his response to our non-acceptance of LIA. Shevoroshkin is someone I helped in some small way to get out of the then Soviet Union and I helped learn how to drive a car after his arrival in the U.S. Given our friendship, I assume there is nothing personal in Shevoroshkin's remarks and that they merely reflect the enthusiasm he has for hypotheses of remote relationships.

9. Greenberg's recollection in this regard is:

I didn't start out to be polemical. Maybe in my earlier life, when I was younger, I might have been, but even then I don't think I ever made any ad hominem attacks on anyone. (Newman 1991:453.)

10. In this setting, it is interesting to cite Greenberg (Newman 1991:457):

... starting out as an Africanist, I was much more aware of the sources. That is, the purely bibliographical side of it is difficult when you start working in a new area. And also, you're not part of the network of people who have been sending each other their most recent publications.

11. Sapir mentioned three on several occasions, although his view of what the three were, and how they might be interrelated, varied somewhat from time to time. Essentially, the three of Greenberg's classification were mentioned already in Sapir's 1916 paper (Sapir 1949[1919]:455). Other variants are given in Sapir 1920, 1921 (cf. Darnell 1990:123, Golla 1984:452, Sapir 1980:83).

12. As Hymes (1971:255) points out:

With support from the Columbia University Social Science Research Fund, he [Swadesh] collaborated with Joseph Greenberg in studies of several linguistic stocks in Africa, Australia, and America (cf. Greenberg and Swadesh 1953, Greenberg 1953, and Swadesh 1952:455). Through Greenberg, the same fund also supported his [Swadesh's] survey of Penutian vocabulary.

In his Boulder paper (Greenberg in press), Greenberg raises the count:

It is clear that Swadesh, like Lamb, was unaware of my paper [Greenberg 1960, given 1956]. The idea [of a threefold classification] was thus "in the air" during this period of roughly four years 1956-1960. If one

adds to this the opinions of Haas, Golla (who apparently endorses it in print in his comments in the CA book review of LIA 1987:657-9), and Kaufman (as just noted), the extent of agreement on this point becomes impressive.

The notion was already well-known and deeply entrenched in American Indian linguistic literature (Campbell in press).

13. Greenberg says that he did not use findings from other fields to argue the correctness of his classification, but:

when you find a convergence of results from linguistics, archeology, and physical anthropology, you can't say that it doesn't strengthen the case for my classification: I think it does strengthen the case. (Newman 1991:457.)

In fact, all the proposed "convergences" are strongly disputed, and, in any case, are irrelevant to the issue of the most adequate linguistic classification (as required by Greenberg's [1957, 1963] own methods and as discussed later in this paper).

14. Greenberg appears to have resorted to rhetoric rather than substance also in his "correction" to Matisoff (1990). Matisoff (1990:139) had observed that Greenberg's methods do not distinguish typological and areal factors from genetic relationship, suggesting that Greenberg would accept the areal features of Southeast Asia as evidence for a genetic relationship among Tai, Hmong-Mieu (Miao-Yao), and Vietnamese. Greenberg (1990:7) objected to this and also got the editor to include a note of correction in a following issue of *Language*. In his objection, he reports that he (in Greenberg 1953) "gave the results of [his] own first-hand review of languages in this area in which Tai, Miao-Yao, and Vietnamese are assigned to separate families: Austro-Tai, Sino-Tibetan, and Austroasiatic, respectively" (Greenberg 1990:7). Nevertheless, even if Greenberg (1953) did group Miao-Yao with Sino-Tibetan, by 1980 Greenberg (see Ruhlen 1987:152-3) was grouping Miao-Yao with Tai (as part of Austro-Tai) and Vietnamese (part of Austroasiatic), all three as branches of his postulated Austric super-family, just as Matisoff had suggested. In LIA (p. 357), Greenberg lists Austroasiatic and Austro-Tai as probably included in a single Austric stock. Clearly, Greenberg's correction of Matisoff misses the substance that in fact his later classifications are fully consistent with Matisoff's claim.

On another front, over and over, Greenberg (cf., for example, 1990:8-9) assails Americanist doubts about his Na-Dene proposal (which attempts to group Athabaskan-Eyak, Tlingit, and Haida, the connection of the last with the others being particularly doubtful). Greenberg assumes that Levine (1979) is the only factor at play, and he criticizes Levine's article severely. For the record, my Na-Dene doubts come from my personal attempt to assess the evidence that has been published and later from Michael Krauss's papers and talks. I cited Levine to acknowledge his earlier publication in this area. However, my Na-Dene doubts are in no way crucially linked to



Levine (1979). The proposal has been disputed on long since before Levine (Greenberg 1953:283). Given this long-standing dispute, Greenberg's manner of resolving the issue is unacceptable. Recognizing Athabaskan-Tlingit-Haida as a "celebrated 'disputed' case", he asserts that "even cursory investigation" shows the relationship not to be very distant, but "evident on inspection" (Greenberg 1953:283). Indeed, were this the case, the dispute would never have arisen in the first place or would have been settled long ago.

15. It will be recalled that Greenberg's African classification was not so fully embraced by British, German, and other non-American Africanists as it was in the U.S.

16. Specifically, among Greenberg's African proposals, the following errors and indeterminacies should be borne in mind. In **Afro-Asiatic** (Afrasian), Cushitic and Omotic (formerly called Sidama or West Cushitic) are disputed (Hetron 1980, Bender 1987, Fleming and Bender 1976:33-39). The great diversity within Omotic makes it a questionable entity for some (cf. Fleming and Bender 1976:46). **Nilo-Saharan** is disputed, the most controversial of Greenberg's African big-four groupings (Gegersen 1977:121, Fleming and Bender 1976:53, 1991). Bender (1971, 1991:19, Fleming and Bender 1976) supports a markedly different classification of Nilo-Saharan languages. Greenberg had six branches of Nilo-Saharan (Songhai, Saharan, Maban [Mabaan], Fur, Coman [Koman], and Chari-Nile [a.k.a. Macro-Sudanic]). Especially different are the place of the Mabaan, Koman, Nara, Surma, and Turkana branches. Bender has no equivalent of Chari-Nile or East Sudanic (Bender 1971, Fleming and Bender 1976:54-8). The Chari-Nile hypothesis has been especially controversial (Goodman 1970, 1971, Thelwall 1982:44, Bender 1991, 1993). The "Mao" languages (placed with Koman and Gumuz) are not Nilo-Saharan at all, but have been reclassified as part of Omotic (Bender 1983:259). Schadeberg (1981) indicates that there is as much evidence for placing Kadugli languages in Nilo-Saharan as there is for Greenberg's assignment of them to Kordofanian. Bender (1983:289) wonders whether Koman and Kadugli may be connected. As for Niger-Kordofanian, the position of the Mande languages as Niger-Congo or as Kordofanian is still unsettled (Welmers 1973:17). Within Mande, the place of Bobo-Fing (Sya) is in doubt; the status of the Gur, Kwa, and Benue-Congo languages also presents problems; the assignment of Kru to Kwa is questionable, as is that of Ijo to Kwa (Welmers 1973:17). Greenberg's Khoisan (one of his four major groups) is perhaps the most unaccepted; Fleming (1983) argues against Sandawe and Hadza in Khoisan (see also Westphal 1971).

17. Even Ruhlen (1987), a strong support of Greenberg's other classifications, seems not to accept fully the Indo-Pacific hypothesis, since another classification is followed in Ruhlen's book.

18. Another is the lack of acceptance of his Miao-Yao/Sino-Tibetan/Austro-Tai.

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Nichols, Johanna: *Linguistic Diversity in Space and Time*. Chicago: The University of Chicago Press, 1992. 358 pp. ISBN 0-226-58056-3. Price: \$45.95.

Reviewed by MERRITT RUHLEN

In this book, Nichols proposes to investigate human prehistory not with the traditional tools and methods of comparative linguistics, but with a new approach called "population typology." Her goal is "detecting affinity at great time depths and describing early linguistic prehistory" (1). The justification for this new approach is that "the comparative method does not apply at time depths greater than about 8000 years" (2). Thus, in place of cognate words — all of which have allegedly disappeared after 8000 years —, Nichols uses just ten grammatical features ("head/dependent marking, complexity, alignment, word order, PP's, inalienable possession, inclusive/exclusive pronouns, plurality neutralization, noun classes, and numeral classifiers" [259f.]), which she herself notes are not independent of one another. A population biologist might wonder about the feasibility of using just ten interdependent features from a single area of language structure to reconstruct all of human prehistory. Most linguists will wonder about the feasibility of using typological traits at all in the investigation of genetic affinity, after Greenberg's demonstration of their absurd consequences in Africa.

Putting aside for the moment these qualms, what are the results of Nichols' study? Does she confirm the validity of Greenberg's African classification? What is the import for the Nostratic, or Eurasiatic, families that seek to connect Indo-European with other Eurasian families? And what about Amerind, Greenberg's recently proposed family uniting all Native American languages, save Eskimo-Aleut and Na-Dene? Actually, Nichols' book does not explicitly discuss any of these matters, and the results of her study are so nebulous and imprecise that families as sharply delineated as Eurasiatic, Amerind, or Nilo-Saharan are invisible to her methods. So, too, would be Indo-European, Uralic, Sino-Tibetan, Austronesian, Afro-Asiatic, or Niger-Congo for that matter, but Nichols makes no attempt to demonstrate that population typology is capable of identifying even such obvious and universally recognized families. Apparently population typology only works before 8000 B.P., while the comparative method only works after that date.

Nichols' principal discovery is that "four features — inclusive/exclusive oppositions, plurality neutralization, adpositional phrases, and inalienable possession — have distributions which take the form of global west-to-east clines, and noun classes may be a fifth" (206). However, the reader's excitement over this discovery will certainly be tempered by Nichols' admission that "these distributions can be called west-to-east only in the sense that the Pacific behaves as though it were east of the New World. The areas of the Pacific are



separated from the Old World by more miles of open ocean than the New World is, and it would be possible to weight the distance of a water mile so as to derive a more eastern adjusted geography for the Pacific" (206f.).

Are there really any clines at all? I think not. Let us take a closer look at the putative inclusive/exclusive cline, which Nichols considers "a garden-variety instance of stabilization." According to Nichols' theory, the origin of this particular cline is roughly as follows. She assumes an initial population in Sahulland (New Guinea and Australia, before they were separated by a rise in the sea level) that had differing frequencies of the inclusive/exclusive feature, somewhat higher than 50% in the south, which became Australia, somewhat lower than 50% in the north, which became New Guinea. Subsequent to the isolation of New Guinea and Australia, the frequency of this feature reached an equilibrium of 100% in Australia and 0% in New Guinea. This, in turn, is part of a worldwide cline, with highest frequencies of the inclusive/exclusive in the Pacific (which, because of the water miles, is thought of as in the Atlantic), the lowest frequencies in the Old World, and frequencies intermediate between the two in the Americas.

There are, however, some problems with the above explanation. First, I know of no linguist who considers the Austronesian inclusive and exclusive pronouns to be historically connected with the Australian inclusive and exclusive, much less these two systems with the other inclusive/exclusive patterns found around the world. All the evidence indicates that these are *historically independent* creations of the same structural type that have nothing whatsoever to do with one another. This is after all what typology is all about: structural similarities that are historically independent. Second, there is a very good reason — natural selection — why gene frequencies stabilize at different values in different regions, but natural selection does not operate on language. What force, then, is responsible for the stabilization of the inclusive/exclusive feature at different rates in different regions? That topic is never broached.

The global clines are not, however, the only discoveries of population typology. Other revelations include: (1) The presence of noun classes in Niger-Kordofanian is due not to the process of grammaticalization proposed by Greenberg, but to "location in or near a hotbed" (139). "... geographical factors suffice to explain the fact that gender exists at all in the Niger-Congo stock: Africa is a class hotbet, and we would expect to find noun class systems in this family" (140). (2) "For the New World areas, affinities are mostly within the New World, except that eastern North America is peripheral and isolated, with more affinities to New Guinea than to the rest of the New World" (224). If this were true, then the Algonquian languages would be closer to New Guinea languages than to Wiyot and Yurok languages located on the northern California coast. Yet Wiyot, Yurok, and Algonquian are universally recognized — except by Nichols, apparently — as forming a valid family. (3) "... rapid circum-Pacific colonization ... including the first colonization of the New World ... was underway by about 35,000 years ago" (228). It

would appear, however, that these first Americans were extraordinarily neat, and left no trace in the archeological record of their first 20,000 years in the Americas. (4) The ancient Indo-European homeland is to be located neither in Anatolia nor north of the Black Sea — the two favored homelands of traditional comparative linguists — but at some unspecified Asian location "east of the Urals" (236).

There are problems with this book that go beyond its substantive fantasies. Despite the assertion on the cover jacket that this book "will be of crucial interest to linguists, archeologists, [and] population specialists", the book is essentially unreadable to anyone without a thorough background in linguistics and linguistic typology. Terminological jargon proliferates on every page, much of it of Nichols' own creation, and few archeologists are likely to comprehend casual references to such linguistic terms as "pro-drop," "Aux," "neutral dominant alignment," or "Aktionsarten," much less Nichols' own creations of "macrogender," "hotbeds," and "spread zones." Indeed, in her exuberance to coin new linguistic terms, she has even added three new words to the English language: "dispreference," "taxonomized," and "taxonomization."

Inasmuch as one must wade through over 200 pages of such typological jargon before the subject of human prehistory even appears, it seems more than likely that most, if not all, archeologists and molecular biologists will skip these pages and jump to the book's conclusions. There is a peril here too, however, for Nichols eventually concludes (274f.) that there were three stages in the spread of modern humans over the earth: (1) expansion out of Africa around 100,000 years ago, (2) a second stage of expansion to Australia, New Guinea, and the Americas between 60,000 and 30,000 years ago, and, finally, (3) a third stage of expansion beginning with the end of the glaciation around 10,000 years ago. The reader who skips the typological sections (which is most of the book) will assume that these sections provide some evidence for this three-stage dispersal theory. In reality, there is not a scintilla of evidence for any of it, and the whole scenario seems simply to have been grafted onto the rest of the book because it fits the emerging archeological and genetic perspectives.

The book is really unreadable even for linguists, and every review I have seen of this book — all by linguists — mentions its "difficult" nature. Virtually every page contains passages that are inscrutable. For example, to explain why Greenberg was so successful in his African classification ("a paradigm case of scholarly success" [5]), but not in his New World classification. Nichols proposes that "the languages of Africa prove to have been underanalyzed raw data for comparative work, while those of the New World proves otherwise" (5). Or consider: "Diversity of a particular kind may even be regarded as the state to which a group of languages will naturally revert if left undisturbed" (23). Or: "Both noun classes and numeral classifiers are hotbet phenomena, and their hotbeds are smaller than continental in size. But the propensity to contain hotbeds has a geographical scale of its own" (199). Or: "the Old World has less internal diversity than either of the other macroareas. In such cases it is sometimes difficult to tell

active areality on the part of the Old World from default unity of the Old World where diversity is greatest in the colonized areas" (205).

This book is based on an absurd assumption — that the comparative method in linguistics is only capable of discovering the obvious: Indo-European, Uralic, Dravidian, Bantu, Algonquian, and other language families of recent origin. The initial absurdity is compounded by additional absurdities on almost every page, but couched in such obscure — and obscuranist — jargon, buttressed by no less than 96 tables, and validated by numerous chi-tests, that to an outsider the work appears one of great erudition, rather than the pretentious nonsense it really is. This is a work, not of science, but of science fiction, and it tells us nothing whatsoever about human prehistory.

Vitaly Shevoroshkin (Ed.). 1991. *Dene-Sino-Caucasian Languages*. Bochum: Universitätsverlag Dr. Norbert Brockmeyer.

Reviewed by NEILE A. KIRK and PAUL J. SIDWELL

This book is the fourth collection of materials from the First International Interdisciplinary Symposium on Language and Prehistory held at Ann Arbor in November 1988. The title alone certainly promises much, but alas the content presents a very a mixed bag. Comprising four parts, the first two are short landmark papers, by Starostin and Nikolaev, respectively. The third is a substantial pastiche, a sort of "collected works" of John Bengtson. The last part, basically a huge list of reconstructed North Caucasian (NC) roots, is somewhat problematical as it comes without etymologies, sources, or any discussion of the correspondences involved.

Vitaly Shevoroshkin gives a four-page editorial introduction (pp. 6-9), which is basically a browse over 13 apparent examples of "identities" or "semi-identities" between Sino-Caucasian (SC) roots and Salish, a (possibly) Na-Dene language of North America. The ambiguous notion of "semi-identities" gives Shevoroshkin considerable license. For example: without having had the regularity of the correspondences explained, it is not immediately obvious on what basis the Salish single syllable *\*qəX-* "horn" should be compared to SC *\*qwVrHV* "horn". Yet on the basis of a few such "semi-identities" we are treated to the notion that this data is so good that it seems to contradict:

the well known thesis that the "best" cognates between proto-languages are those represented by "non-trivial" sound correspondences (not appearing on the surface). (p.9)

The historical linguist's most important tool is a firm command of logic, yet this is lacking in Shevoroshkin's editorial comments. It is obvious that even if these "semi-identities" are cognate, no law is being violated. Good look-alikes are easily spotted, and so will be over represented early in any reconstruction. Furthermore, in any natural phenomenon, one expects to see the evidence of the randomness of change. Chances are that there will be some excellent trivial correspondences if the relationship is real, perhaps even more than the number of accidental look-alikes one would expect in any case. But trivial or not, the correspondences must be regular to be meaningful, a point not mentioned by Shevoroshkin (and even denied by Bengtson and Ruhlen 1994).

This is not the only example of Shevoroshkin's rather casual attitude within his introduction. Astoundingly, we are informed that the fourth part of the book presents us with a list of 20,000 NC roots. Even a grade school student would quickly work out that 190 pages with an average of 22 roots per page will never yield 20,000, and a Russian linguist of the Nostratic school should intuitively know that even by the most generous glottochronological reckoning only a few thousand roots at most will be recoverable in a language family as old as NC.

Shevoroshkin is doing a great job assembling these papers for publication, and we congratulate him, but his editorials do nothing for these volumes. He should let the contents speak for itself.

The book really begins with William Baxter's English translation, under the title "On the Hypothesis of a Genetic Connection between the Sino-Tibetan Languages and the Yeniseian and North-Caucasian Languages", of Starostin's paper "Gipoteza o genetičeskix svjazax sinotibetskix jazykov s jenisejskimi i severnokavkazskimi jazykami". Originally published in the collection *Drevnejšaja jazykovaja situacija v vostočnoj azii* (Institut vostokovedenija AN SSSR, Moskva, 1984) (*The earliest linguistic situation in east Asia*), is arguably the highlight of the volume. Starostin's work represents classic reconstruction methodology, making it a pleasure to read. His reconstructions should be studied by all as lessons in how to get the job done by sticking strictly to the logic of reconstruction.

The paper opens with a brief introduction, and then presents the tabulated correspondences for the obstruents. There follows discussion of syllable structures, the behavior of sonorants, and conditioned changes. This is then followed by the comparative lexicon according to the Swadesh 100 list and further cognate sets based on semantic fields. The paper closes with discussion of glottochronological datings and possible external relations.

Given this very tidy structure, with tabulated data, transcriptions in phonetic script, and the fact that even in the original the Swadesh list was organized in alphabetical order of the English glosses, any serious non-Russian speaking linguist would have no trouble with the original paper. Obviously produced for American consumption, it is a sad commentary on the profession in that country that it is felt that such papers have to be translated in order to make them more accessible (note that even *Mother Tongue* refuses to publish contributions that are not in English). But even translations are problematic. Given the almost total lack of reading culture in American society, it is doubtful that even this strategy will yield positive results. In this context, it is pleasing to see that Starostin's original Russian glosses are included in brackets so that we can tell what they really are meant to be rather than necessarily struggling all the time with translations of translations. Baxter renders Russian "noga, kopyto" as "leg, hoof" (p. 25). *Kopyto* indeed means "hoof", so that the other meaning of *noga*, namely "foot", should have been included in addition here. Unfortunately, there are many misprints. For instance, the Russian word for "to call" should read *zvat'* instead of *svat'* (p. 37). On page 40, the Russian adjective translated as "fat" should read *tolstyj* instead of *toltsyj*. The misprint *drevnejšaja* (p. 170) should read *drevnejšaja* (most ancient).

Strangely, we find *kiška* twice as the transliteration — in accordance with the system of transliteration explained in the translator's preface of the Russian word for "intestine", but *kishki* on the line between as the word for "intestines" (p. 24). Another curiosity is the reconstruction for *čemila, tuš* (p. 33) "ink, India ink". Are the speakers of Proto-North-Caucasian and Proto-Sino-Tibetan — by Starostin's dating no later than the fourth to sixth millennium BC — really supposed to have

had India ink? Of course not, so have we been given a simple dictionary gloss and not a *translation* of the reconstructed meaning?

There is of course no great semantic difficulty in comparing, as Starostin does, forms glossed as "bird [ptica]" with one glossed as "owl [sova]" (p. 18). It is problematic as to whether there can ever be a definitive reconstruction of the meaning of the Proto-Sino-Caucasian word whose Proto-North-Caucasian cognate means "kidney [pocka]", whose Proto-Sino-Tibetan cognate means "lung [legkoe]" and whose Proto-Yeniseian cognate means "spleen [selezenka]" (p. 26). Even where the cognate meanings bear a clear resemblance to one another, their differences can sometimes seem to preclude a semantic reconstruction going further back, such as where we have a Proto-East-Caucasian root glossed as "oath, vengeance [kljatva; mest']", the Proto-Sino-Tibetan cognate glossed as "to curse, to insult [rugat', oskorbljat']" and the Proto-Yeniseian cognate glossed as "anger, to be angry [gnev, serdit'sja]" (p. 34). On the whole, in his comparisons of proto-forms with proto-forms, there are not the fanciful semantic developments postulated in the work of more reckless lumpers. Quite reasonably, Starostin reconstructs a Proto-East-Caucasian root for "to freeze; ice [zamerzat'; led]" whose cognate in Proto-Sino-Tibetan means "hail; rain with snow [grad; dožd' so snegom]" and with a Proto-Yeniseian cognate meaning "to freeze, to get cold [merznut', stynut']" (p. 30). Also he compares his Proto-Yeniseian word glossed as "hip, thigh [bedro, ljažka]" with a Proto-Sino-Tibetan word for "leg, calf of leg [noga, ikra nogi]" and two Proto-Lezgian forms glossed as "knee [koleno]" (p. 24).

The semantics however do get a little less constrained when Starostin occasionally compares Proto-North-Caucasian and Proto-Sino-Tibetan with, instead of proto-Yeniseian reconstructions, words in modern — or, at least almost modern — Yeniseian languages. Starostin glosses Ket *qəʔq* as "debris drifting on a river at flood-time [musor, nanesennyj rekoj v polovod'e]". This is supposed to be cognate with a Proto-East-Caucasian reconstruction glossed as "dirt /filth, pus, mold [grjaz', gnoj, plesen']" and a Proto-Sino-Tibetan form glossed as "dirt /filth, dung [grjaz', navoz]" (p. 30). The Ket term *qəp-ku* "calves of the legs [ikry nog]" is supposed to be cognate with the Proto-East-Caucasian word for "paw [lapa]" and the Proto-Sino-Tibetan for "fork of the legs, groin [razvilka nog]" (p. 24). On page 29 he compares Kott *keri* "grass [trava]" with a Proto-Sino-Tibetan word for "reed [trostnik]" and a Proto-North-Caucasian word for "grass, stem [trava, stebel]". On page 26 he compares a Proto-East-Caucasian word for "armful [oxapka]" and a Proto-Sino-Tibetan form glossed as "to hold with two hands [deržat' dvumja rukami]" with a Proto-Yeniseian form glossed as "fathom / armspan [sažen']" and also the Kott form *ham-al* "armful [oxapka]". There is a rather curious comparison on page 39 of Ket *qə:j* "neighboring [sosednij]" with a Proto-East-Caucasian and a Proto-Sino-Tibetan form, both glossed as "alien [čužoj]". Starostin puts a question mark before "Jug kutuk 'kennel [konura]'", which he compares with a Proto-North-Caucasian and a Proto-Sino-Tibetan form, both of which

are glossed as "enclosure, fence [zagon, ograda]" (p. 31). He compares Kott *sāk* "frozen snow-crust, icy crust [nast, ledjanaja korka]" with a Proto-North-Caucasian form glossed as "drizzle, hoarfrost [izmoros', inej]" and a Proto-Sino-Tibetan form glossed as "rain [dožd]" (p. 30). He compares Kott *tuk* "saliva [sljuna]" with a Proto-Sino-Tibetan synonym and two Proto-Nakh forms glossed as "saliva, spittle [sljuna, plevok]" (p. 25). The Ket word *ag-di* "mouth cavity [polost' rta]" is compared with a Proto-East-Caucasian form glossed as "hole, window [dyra, okno]" and with two Proto-Sino-Tibetan forms, one glossed as "opening (also mouth, door) [otverstie (tž, rot, dver')]", the other as "hole [dyra]" (p. 31).

In one instance, Starostin gives a modern Tibetan form, namely *theb* "thumb / big toe [bol'šoj palec]" as cognate with a Proto-East-Caucasian word for "finger / toe [palec]" and a Proto-Yeniseian word for "thimble [naperstok]" (p. 24). There is even an instance where he compares a Proto-East-Caucasian form glossed as "stick, piece of a log [palka, kusok brevna]" with two words from modern languages, namely Tibetan *m-khar* ("pole, stick [žerd', palka]" and Jug *kɔʔl* "stump [pen']" (p. 29).

Following Starostin's paper is Sergej Nikolaev's paper "Sino-Caucasian Languages in America — Preliminary Report", which largely assumes familiarity and agreement with the contents of Starostin's paper and confidently announces that "it has been proven" that the North American Na-Dene language family is related to Starostin's Sino-Caucasian macro-family. Nikolaev's methodology and presentation is pleasingly similar to Starostin's, although some remarks are worth making. The comparison is made between NC and Proto-Eyak-Athapaskan (PEA). NC is only a SC sub-grouping, and PEA is similarly a Na-Dene sub-grouping, which therefore limits significantly the common Dene-Caucasian lexicon that comparison of these families could reveal. In any case, the results presented are remarkably good, much better even than Starostin's recently presented evidence for the Altaic pedigree of Japanese (1991). In considering the comparative vocabularies offered by both Starostin and Nikolaev, it is worth recalling Sapir's remark all those years ago when he began investigating the linguistic connections between the Old and New Worlds:

If the morphological and lexical accord which I find on every hand between Nadene and Indo-Chinese is "accidental", then every analogy on God's earth is an accident... For a while I resisted the notion. Now I can no longer do so. (Sapir 1921)

A great deal of the book (pp. 67-172) consists of papers by the American John Bengtson. This section opens auspiciously between pages 66 and 67 with a hand-drawn map of the world — or most of it, anyway: Antarctica, Australia, and part of South East Asia made way for the caption "Distribution of Sino-Caucasian languages" with an accompanying key to the particular areas. Not that this matters, since the southern hemisphere does not seem to have received any of the Sino-

Caucasian languages anyway. Unfortunately, the presentation is amateurish to an extent that does not dignify a scholarly work. The look of the map is positively Tolkienesque, including references to "ancient kingdoms" etc. Fortunately, we do not find captions like "Land of the Giants" or "Trolls go there".

The majority of the Bengtson pastiche consists of "SINO-CAUCASIAN ETYMOLOGES" (pp. 84 to 129) and other papers listing suggested "MACRO-CAUCASIAN" etymologies. On top of pointing out many apparent lexical parallels among the DC languages, Bengtson's work really falls into two kinds: in first place collecting and synthesizing much of the significant body of scholarship that has preceded him, and secondly selling his own twist to the Sino-Caucasian hypothesis, namely Macro-Causasic (MC). Without a doubt, if Bengtson has seen further than others, it is because he has stood on the shoulders of giants (apologies to Isaac Newton).

While accepting that at some level the northern hemisphere is literally ringed by Dene-Caucasic languages, Bengtson asserts that Basque, North Caucasian, and Burushaski represent a genetic grouping which are more closely related to one another than they are to Sino-Tibetan or Na-Dene. To support this, some morphological evidence is presented and what must be considered preliminary phonological correspondences are suggested. The significant number of lexical parallels given may or may not support the sub-grouping hypothesis, unfortunately no lexicostatistical analysis has been presented yet (although the authors of this review are aware of work in that direction presently under way).

Even the MC hypothesis is not really new (it smells a lot like Shevoroshkin's western branch of SC [1989:15]) as anyone familiar with the history of Soviet linguistics would be aware. Trombetti is normally given credit for linking Basque, Caucasian, Sino-Tibetan, Yeniseian, and Na-Dene in the 1920's (Trombetti 1926), but Soviet linguists, including N. Ja. Marr discussed and supported such theories at this time and later, including Etruscan in the equation.

Some of Bengtson's etymologies are interesting, especially when he uses them to push his "substrate" charabanc. Swedish *hår*, German *Haar* and English *hair* have not eluded Indo-European explanation, notwithstanding Bengtson's claim to the contrary (p. 171), although "explanation" is perhaps ambiguous here. Perhaps Bengtson should take a look at a few more etymological dictionaries. For the etymology of *hår*, a look at a standard Swedish etymological dictionary is illuminating. The Germanic words for "hair" are related to such other Indo-European words as Greek *keirō* (cut), Russian *šerst'* (wool) and *kosa* (plait, braid) (Hellquist 1980:381). Also related to these words is Russian *česat'* (to comb) (Šanskij 1982:344). Despite Bengtson's assertion (p. 171), there is an Indo-European etymology for English *lamb*, German *Lamm*, Swedish *lamm*, which have in fact been connected with Greek *ellōs* (deer calf) and Old Church Slavonic *jelenъ* (deer) (Kluge 1989:426), whose reflex in modern Russian is *olen'*. The Finnish word *lammas* (sheep) is a borrowing from Germanic \**lambaz* (sheep) (Serebrennikov 1968:47). English *dew*, German *Tau*, and Swedish *dagg* may be connected with Latin *fūmus* (smoke, steam) (Serebrennikov 1968:723). In reality,

Swedish *gata* (street), German *Gasse* (narrow street, lane, alley), English *gate* ("way" from Old Norse) seem to be connected with Greek *khézō* (*shit*), Armenian *jet* (tail), and Armenian *djes* (*shit*) (Jóhannesson 1956:328). He claims (p. 168) that Spanish and Portuguese *caracol*, Catalan *caragol*, and French *escargot*, which all denote "snail", are among the "traces" of Macro-Caucasian languages left in Europe. Viewed from the context of Russian and Soviet linguistics, Bengtson's speculations about a "Macro-Caucasian" substrate in Europe look just like our old friend, the Japhetic theory of N. Ja. Marr. Marr is usually dismissed in the West as some kind of Stalinist crank, but in fact he died in 1934, so the bulk of his work was done well before the height of the terror. It would be unfortunate if the work he did in those last few years of rather strained political environment were all that he was remembered for. An outstanding linguist, he was born of a Georgian mother and Scottish father (Vogt 1970). Unfortunately Marr's later disciples tyrannized Soviet linguistics until his fellow-Georgian J. V. Stalin spoke out against them in 1950 (Stalin 1967:145). Bengtson does us all a great service by constantly reminding us of how much work has gone before. His lists of references alone are enlightening, although very curiously Marr does not seem to appear among them.

Old Japanese *Fara* "belly", referred to by Bengtson in the context of "possible remote connections" of his Proto-Macro-Caucasian word for "intestine" (p. 95), is well-known in its modern form *hara*, which occurs in *harakiri*.

Bengtson sometimes likes to use analogies to support his case. He cites (p. 93) the example of Old Swedish *fläsk*, which he glosses as "meat, flesh, fat flesh, grease" as a semantic parallel in support of his Proto-Sino-Caucasian "flesh" etymology. As a matter of fact, the word *fläsk* is still used in modern Swedish but with a more restricted meaning, in the sense of "pork" (Gravier 1975:381). He uses the comparison of Norwegian *far min* (my father) with Swedish and Danish *min fa(der)r* as a typological parallel to the possible variation of the position of the possessive at the time of the hypothesized breakup of Sino-Caucasian (p. 75). He cites, for the purpose of typological comparison, the example of Old English *fō-* and Old Icelandic *fá* (to catch, seize) (p. 93).

The final part of the volume, titled "Reconstructions", comprises the list of NC roots by Sergej Nikolaev and Sergej Starostin, mentioned above. It would have been better if the Russian originals had been given of the glosses of the circa two thousand items — not to mention all those other bits and pieces that go together to justify a reconstruction and make it usable to other scholars. It is pleasing in the extreme that Nikolaev's mighty manuscript is now in print (Starostin and Pejros's massive hand written etymological dictionary of Sino-Tibetan, circa 1984, is still gathering dust, another tragedy of Soviet proportions), but it is also extremely frustrating that it should appear in this contextless form. One little bone to pick, the transcription guide to the list gives a symbol for a voiced glottal stop! It is well known that Caucasian languages make use of all possible articulations, but we are battling to find precedent for this one.

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## QUICK NOTES

1. It was recently announced that the Environmental Protection Agency (EPA) has financed some gaseous, but potentially ugly, research (*Boston Sunday Globe*, May 29, 1994, p. 13). Since half of a million US dollars are involved, we should examine these grants. Says the *Globe*, "Having spent \$300,000 to study whether cow flatulence contributes to global warming, the government is now spending even more to analyze bovine belches." The trick is to round up a bunch of cows and fit them with "special breathing devices that measure the amount of methane cows release when they burp." It will be a neat trick to capture the farts, no doubt. The research is to be done at Utah State, but was started at Washington State University. The Washington State researchers "tried to disarm jokes about cow flatulence, saying 95 percent of the methane comes from the front end of the cow." Also "Utah State range livestock nutritionist Ken Olson ... reiterated the significance of the research. 'Methane produced by cattle appears to be a consequential factor in global warming', he said. Some researchers estimate that confined cattle produce about 20 percent of global methane emissions." Now it seems to me that 10 million Australian beer drinkers could take the lot of them!

But really, mes amis, wouldn't half a million bucks do wonders for prehistoric research! Linguistic especially?

Curiously enough, the *Globe* ran photos of a hot air balloon jubilee, right next to the article on Utah State's research.

2. Completing his first round of excavations on agriculture in China, Scotty MacNeish announced that his team had found the *earliest rice* ever, the oldest site for domestication in China, one of the oldest sites with pottery in the world, and one of the oldest sites showing domesticated grain anywhere in the world. The sites were two caves in Jiangxi (Kiangsi) province, not too far from Nanchang. The cave deposits appear to go back to 20-25 kya and may show the transition from hunting and gathering to first agriculture which shows up circa 11,000 BP. Kwang-Chih Chang of Harvard reportedly said: "Wherever he goes, things happen. What MacNeish has found is important and promising but needs a lot more work." Scotty also pointed out that Chinese excavations earlier "in the neighboring province west of Jiangxi [Hunan - HF] have produced evidence of rice agriculture between 7,500 and 9,000 years ago, and in the next province to the east [Zhejiang, earlier Chekiang - HF] there is similar evidence from 6,500 to 7,500 years ago. So we're bracketed. We're right in the middle." This would make sense if the growing of rice started in Jiangxi and gradually spread outward. Also MacNeish said that Yan Wenming, chairman of the Archeology Department at Beijing University and the Chinese co-leader of last year's dig, had predicted that this was the area where the origins of rice would be found. The pottery was described as "a few scraps of crudely fired clay... pretty crummy stuff... much like the dried mud that comes off your boots." [Note: China will *not* accept second place in anything!] [This was all reported in the *Boston Globe*, July 4, 1994]

3. The *Ice Man of the Alps* has had his name altered a bit, but finally the genetic data got analyzed and now we know who he was related to. Although so much of his DNA was degraded, it was still possible to determine his mtDNA type. It showed that "the mitochondrial type of the Ice Man fits into the genetic variation of contemporary Europeans and that it was most closely related to mitochondrial types determined from central and northern European populations." The report was made by 13 authors who we will refer to as Oliva Handt et al.; it was entitled "Molecular Genetic Analyses of the Tyrolean Ice Man", and it appeared in *Science* 264:1175-1178 (17 June, 1994).

Before people open their jaws to yawn at this amazing result, they should realize that in effect we have evidence for 5,000 years of population sameness or continuity between the Alps and northern Europe, especially Denmark and Germany. There is an excellent chance that this fellow walked into northern Italy from Austria or southern Germany — an archeological conclusion — and was culturally connected to late Neolithic or early Bronze Age cultures of north Italy and Austria who were *Pre-Indo-European*. By extension we could argue, as some archeologists have (e.g., Ben Rouse in the 1950s), that the Germanic area seems more descended from native hunter-gatherers than from either Danubian farmers of Anatolian type or Aryan cowboys from the east.

By the way: there have been persistent reports that the Ice Man had been bugged, i.e., that another man's semen resided in the frozen rectum. Oliva Handt et al. denied any evidence of "emasculatation" and never mentioned buggery. Too bad, we might have had two mtDNA samples for the price of one!

4. New data from Australian archeology show that it is time to change our notions of what aboriginal society was like before the English practiced ethnic cleansing there. Much *larger populations* and much *more sedentary* groups seem to have been the norm in the temperate southeast where most of modern Australia lives. As reported by Graeme O'Neill in *Science* 264, p. 1403, 3 June 1994, as "Cemetery Reveals Complex Aboriginal Society", Colin Pardoe and Harvey Johnston, researchers from New South Wales, almost tripped and fell over a vast sand dune with an estimated 10,000 *skeletons* with occupancy of a site going back at least 7,000 years. Not small nomadic bands but groups of 100+ circulating within a smallish area of maybe 20 km<sup>2</sup> and hunting a rich local habitat with "a diverse larder of plants and animals". As has been, and is being, argued by both ethnologists and archeologists, many hunting and gathering societies in favorable temperate habitats — like the Northwest Coast of North America or Europe before the Neolithic — lived quite well and prospered. Why bother with the arduous work of farming if you don't have to?

5. Upward mobility is not characteristic simply of struggling Calvinists and sports heroes. It is also true of Alpine plants. In places as far apart as Alaska and Austria, high altitude or cold climate plants, including flowers, have been observed moving

upwards in altitude as global climate has warmed only a little. Top altitudes are getting crowded, and some species will die out as warming increases even a little. The report was in the *New York Times*, C4, June 21, 1994.

6. A human fetus is inherently female and only a special gene can make the baby a boy. So saith an older belief. But recent research says it is *false*. As reported in *New York Times* C5, August 30, 1994, "Biologists are Hot on Track of Gene that Determines Femaleness". The older notion (itself modern) was that the "building of a female is a passive business, one that will occur in the absence of any particular reason, while putting together a boy demands the input of the SRY" [the key "maleness" gene on the Y chromosome - HF]. Now it appears that interactions of parts of the X and Y chromosomes are more complex. The recent conclusion is that: "In the new paradigm of sex determination that is emerging, the fetus is roughly female to begin with — just as a child's drawing vaguely resembles the work of Matisse. Whether making a boy or a girl, the final flourishes demand an artist's touch."

7. If you think it is hard to agree on the age of *Homo sapiens* in Israel, try your luck at dating the beginning of everything — like the universe. When two bold astronomers disagree, then we have trouble. That is what is now the problem with the age of the universe. One new set of dates obtained by calculating the "Hubble Constant" has set the dates between 7 billion and 11 billion years ago. However, the age calculated from the ages of the oldest known stars is about 16 billion years. However the astronomers may settle this question, it is reassuring to prehistorians to know that we are not the only ones with dating problems. Perhaps we could lend Professor Ringe to astronomy to help them figure things out. Well, they have enough problems...

8. The *St. Petersburg Journal of African Studies* has another volume, #1, which I've not seen. Volume 2 arrived this summer; after many address changes, they found me. This is quite a solid piece of work and the first two (and key) articles feature six long rangers, most of the original Muscovite band who inspired *Mother Tongue* to begin with plus our redoubtable and excellent colleague, Igor Diakonoff. The first article is "Historical Comparative Vocabulary of Afrasian". Igor is team leader, joined by Anna Belova, Alexander Militarëv, Viktor Porkhomovsky, and Olga Stolbova. A. Chetverukhin completes the team. This article is the first of a series, beginning reasonably with Proto-Afroasiatic \**p*, \**p'*, and \**f*. It will continue through what will probably be a world standard for Proto-Afroasiatic. I found the etymologies to be formidable, careful, and convincing. Naturally, I disagreed with some etymologies but with a kind of "well, maybe they are right" attitude. I also struck gold on page 23 with No. 53 \**fu/i* "breast, teat". The vowels are voiceless. Since I had not found a cognate for Ongota [7ééfe] "milk" and [fiíya] "milk!" in the southern branches, I had postponed looking in the northern sub-phyla for it. But there it was in, e.g., Berber Figig /if/ "breast" or Amharic /fäyā/ "to suck the breast, to drink milk by suckling" or Gafat

/i-fwa-tä/ "milk". Our Ongota informants knew no Amharic. Bravo! StPJAS! May I respectfully suggest that the semantics of the proto-form is more likely to be "suck" or "milk" than "breast"?

The second article was by Vladimir Dybo, one of the three original pioneers in Nostratics. Entitled "Accentuation Processes in the Languages of Teda-Kanuri Group and Problem of Origin of Paradigmatic Accent Systems", it also is to be continued. This is a Nilo-Saharan paper which is guaranteed to delight a paradigm-loving comparative grammarian.

For those wishing to subscribe to StPJAS, I suggest you write to the Managing Editor, Valentin F. Vydrin, Museum of Anthropology and Ethnography, Russian Academy of Sciences, 3, University Embankment, St. Petersburg 199 034, Russia. Telephone (812) 218 08 11 or (812) 218 41 41 or (812) 218 16 72. Fax (812) 218 08 11. E-mail <tseytin@hm.spb.su>. Apparently they have an alternative name = St. Petersburg Association of Scientists, Izdatelstvo Evropeiskogo Doma (St. Petersburg), St. Petersburg, Russia. Or write to Diakonoff at his work address. Institute of Oriental Studies, Russian Academy of Sciences (St. Petersburg). 18, Dvortsovaja nab., St. Petersburg 191 065, Russia. Fax (812) 311 51 01. For most subscribers in most countries, \$37 will get you issues for 1993 and 1994 (together). Add \$2 for postage per issue. Do not send US \$ to St. Petersburg but rather to Man's Heritage Press Ltd., P.O. Box 427, Woodmere, New York 11598 or pay in French francs to Dr. Gerard Dumestre, Institut national des langues et civilisations orientales, 2, rue de Lille, 75007 Paris. StPJAS also has a lot of material on West African languages.

9. Rich new old hominid sites in South Africa. Boycotted for many years internationally, when not scorned informally, South African scientists find more attention being paid to their work. And luckily the *ripe site of Gladysvale* near Johannesburg has opened up recently and appears to be rich in fossils, especially of early man. The fossiliferous soils of South Africa have supplied much of the data for human evolution; it is hoped that the old role will be renewed. (Many of our colleagues from South Africa did not believe in Apartheid and were not in fact scorned by the rest of us, at least in linguistics and anthropology.) For more information, including late-breaking news of new finds, one might contact anthropologist Lee Berger at S.U.N.Y., Stony Brook, New York or Dr. C. K. Brain at Transvaal Museum, Pretoria, South Africa, or Philip Tobias, Paleoanthropological Research Unit at University of Witwatersrand, South Africa. Good fortune may already have smiled upon the excavators!

10. Dr. Randall L. Susman of S.U.N.Y. at Stony Brook, New York, has concluded that tool-making is not necessarily a mark of human evolutionary "progress", not at least if it means that only the mainstream hominids — our lineage — were the tool-makers. His thoughts on the diagnostic significance of *mankind's large thumbs* and their clear association with tool making were outlined in the *Boston Globe* on September 12, 1994. His logic goes like this: Lucy had thumbs like a chimpanzee and a small brain, but she walked upright. Later,

our lineage got larger brains and big thumbs, which are much more adept at making things. However, some of our "stupid" cousins like *Paranthropus robustus* (or *A. robustus*) had big thumbs too. While we have not yet found the evidence of his tool making, in principle he could have. So having great thumbs does not guarantee great tool making, said F. Clark Howell of Berkeley. But the notion that tool making is distinctively human, or implies a big brain, does not necessarily follow would be Susman's reply.

Howell mentioned that the belief in the importance of tools goes back at least to Benjamin Franklin and won't go away. On the other hand, I would argue that paleoanthropologists have made some singularly stupid statements about tool making, including the notion something like "if you can make a tool, you must have a language; therefore toolmaking necessarily implies language."

11. The inheritance of Type I or insulin-dependent diabetes has always been puzzling. Once it was thought one could inherit the diabetes gene, but because of "incomplete penetrance" one would not develop the disease. Or you had the gene, but it didn't work. Now in a fascinating genetic study at Oxford that would delight Gregor Mendel's ghost, Dr. John Todd has studied about 300 diabetic children whose parents did *not* have the disease to figure out how they could have inherited it. His findings were reported in detail in the *New York Times*, C3, September 13, 1994. The upshot is that at least *five genes acting in combination* cause the actual occurrence of diabetes. (This has nothing to do with adult-onset diabetes, which may or may not have a genetic element) The genetics is interesting, of course, but also the detective work in Todd's findings is fascinating.

12. The general notion that *aging* in human beings leads to *cancer* because aging simply increases the chances of genetic mistakes or *mutations* happening, hence cancer, — this notion got specific support from genetic research on a genetic mutation which causes non-Hodgkins lymphoma. (*New York Times*, C5, September 13, 1994) Dr. Gino Cortopassi of the University of Southern California found that the number of mutations in the blood of children was very small, that of adults larger, and that of old people very much larger. Not all the mutations had accumulated enough to give their hosts cancer, but the odds were clearly more likely for the disease to develop. Some researchers have suggested that there is a specific gene for aging and dying, or some specific genes, but that was not tested in Cortopassi's research.

13. More frozen Aryans from central Asia. The *National Geographic Magazine* ran an article on "Pastures of Heaven", pp. 80-103, vol. 186, October 1994. There are many photos and drawings of mummies and cultural apparel from a site called Ukok on the Ukok plateau at 7,500+ feet (2,272 meters  $\pm$ ) elevation. This is almost exactly on the border between Russia and China in the Altai, with the Kazakhstan border very very close. At the head waters of the Argut river, a tributary of the Katun river, Ukok is just over the mountains to the north of

Sinkiang where Aryan mummies were reported by Victor Mair in *Mother Tongue* 21. The archeological research at Ukok had been going on for 4 years when unvandalized frozen tombs were found in 1993. Natalya Polosmak, a senior research fellow of the Institute of Archeology and Ethnography in Novosibirsk, authored the report.

The tomb belonged to Pazyryk culture very common in the Altai (scores of kurgans) and dated to about 2400 BP. The Pazyryk culture is apparently fairly well-known and closely related to the widespread Scythians, or Scytho-Siberians as some call them, who dominated a vast area from eastern Europe to the Altai (if we include the Pazyryk). Thus the Ukok culture had probably been Iranian in speech and not even the earliest of that.

While this excavation was not terribly important scientifically, being more dramatic and colorful than anything else, it did offer unusually well-preserved humans. Not only was it possible to get DNA off of flesh, it was possible to make simple "racial" observations — the mummy had "European features". In addition, both silk, ordinary wool, and felt materials were preserved, as well as art work in tattoos. We wonder how the Ice Princess of Ukok compares genetically with the Ice Man of the Alps.

14. Supposing that the structure of the inner ear might be related to upright posture, researchers Bernard Wood, Fred Spoor and Frans Zonneveld reported in *Nature* this summer that the ear canal bones were different as between apes and most hominids. Specifically, the Australopithecines were more like apes, while *Homo erectus* resembled modern humans. Interestingly enough, they found that of two *Homo habilis* specimens one was more like a monkey than even an ape, while the other was very similar to modern humans. They used "computed tomography scanners" in reaching their conclusions and supposed that "a more precise phylogenetic picture of early hominids should emerge" if they examined more fossils. Sounds a bit presumptuous to me. All the other criteria may be wrong, but the ear bones will tell all! Ah, high tech folks, give a man a better tool and he may strut a bit. Interesting for non-Americans. If you want to read the ultimate in *technology worship*, especially high tech stuff, read the novels of Thomas Clancy, famous most of all perhaps for "The Hunt for Red Oktober". He does action/espionage/small war type novels of the late Gorbachev or early post-Cold War period.

15. "Cave Filled with Glowing Skulls: A pre-Columbian Palace of the Dead". Thus shouted the *New York Times*, C1, October 4, 1994. Another case of dramatic and exciting — indeed Indiana Jones type stuff — but modest scientific import. In Honduras, a large cave in the rain forest, difficult of access, was discovered and examined. It contained many skulls and bones which glowed blue or red because of mineral deposits dripped on them from the cave. The dates were a trifle wobbly but generally in the 1st millennium BC. The culture was non-Mayan and seems to have been a major one. The site is "Cueva de Rio Talgua" about 4 miles northeast of Catcamas in north central Honduras. Who would be a fairly dense population on

the periphery of Copan (Mayan) civilization? Maybe it was Kenneth Hale's Misumalpa or their relatives? For more details, contact Dr. James E. Brady, George Washington University, Washington, D.C., USA.

16. "New Clue to Cause of Dyslexia Seen in Mishearing Fast Sounds", said the *New York Times*, C1, 7, 10, August 16, 1994. Dyslexia which is difficulty in reading or young children's failure to learn to read has ordinarily been approached as a *visual* problem; something must be wrong in the eyes or the visual centers of the brain. Dr. Paula Tallal of Rutgers University (at Newark, New Jersey) have found that the key to the ailment lies in the original difficulty the children had at *learning language itself*. Linking her research to that of Dr. Albert Galaburda, Harvard Medical School, the *Times* concluded that an underlying problem in the "auditory pathway" in the left hemisphere of the brain and most particularly to the "Medial geniculate nucleus" in the thalamus was probably the ultimate source of the problem. Dyslexia is far more prevalent among males than females, and males seem less able to spread language functions around their left hemisphere or over to the right hemisphere than females are. The crux of the matter is the hearing of *fast sounds* (faster than 100 milliseconds or a tenth of a second). While general noise, music, and even long vowels of language are not a problem because they last longer than 100 milli-seconds, language syllables like [ba], [ta], [pa], etc. are faster (circa 40 milliseconds) and thus hard to hear or inaudible. Is this genetic?

17. As Phil Lieberman reminds us from time to time, the study of diseases correlated with the brain tell us much about language in relation to the complex circuitry of our central nervous system. New research on the remarkable children who are fluent in language at the appropriate age but markedly deficient in ordinary intelligence and spatial relations tells us more. These children are known as cases of the "Williams syndrome". Their language development seems indeed to be superior to most children, as is their sociability. Most of them suffer from "super aortic stenosis", i.e., narrow aortas and bad heart valves. The condition is probably genetic. Once again Dr. Galaburda of the Harvard Medical School, as well as the well-known psychologist Bellugi, are at the forefront of the research.

The genetic basis is the "loss of one copy of the gene that makes elastin, a protein that is the chief constituent of the body's elastic fibers, and possibly by the loss of another gene or genes of unknown function that lie next to elastin on chromosome 7. The result of this small genetic loss is far-reaching. There are severe malformations throughout the brain and heart, yet the capacity for language is remarkably unaffected. If anything, language and sociability are enriched. Williams syndrome children, who have distinctive elfin features, are extremely social, verbal, and adept at recognizing faces, but most cannot expect to live independent lives" (*New York Times*, C1, 6, August 2, 1994).

Most of us probably link linguistic fluency, being "well-spoken", and extroversion with intelligence. Yet the Williams syndrome kids do very poorly on intelligence tests

and in exercises such as drawing pictures of animals and handling fairly easy mathematical problems. They often cannot tie shoe laces. Yet they are great story tellers and parents say that "many Williams children have excellent musical ability and enjoy playing instruments and singing songs... Moreover, they tend to be sensitive to certain classes of sounds, some of which are disturbing while others turn into obsessions. One boy loves vacuum cleaners. Another adores power lawn mowers. And these children's hearing is acute; they can detect faint sounds in the distance, long before others notice them... While their hearing tests are in the normal range, neurons in the auditory cortex are exceptionally excitable."

Bellugi and others thought the brains might be asymmetrical. "After all they had good language ability, which is controlled predominantly in the left side of the brain, and poor spatial ability, which tends to originate in the right side. But, in one of the biggest mysteries of Williams syndrome, the children are extremely adept at identifying faces — a difficult spatial task carried out by the right hemisphere."

"But Dr. Galaburda's autopsies and numerous brain-imaging studies present a more complicated picture. There are no obvious lesions in the left or right sides of the brain, she said. Instead, the cerebral cortex is shrunken on both sides and fibers connecting the left and right hemispheres are thin or diminished. Moreover, these abnormally small brain regions are packed with an excessive number of neurons."

"During development, the brain produces an overabundance of neurons and then eliminates the excess through programmed cell death... At the same time one circuit ... seems spared. The frontal lobes, medial temporal lobes that include part of the auditory cortex and the neocerebellum are closer to normal size, Dr. Galaburda said." My! my!

18. Late breaking news related to #17 is that the *thalamus* has been linked to *schizophrenia*. Reported in the *Boston Globe* (October 17, 1994, p. 28), the burden of the story is that schizophrenia had previously been analyzed in terms of specific patterns, such as delusions and social withdrawal, which were then correlated with different sectors of the brain. That approach did not pay off. Now a more holistic approach, viewing schizophrenia as a package, has succeeded in linking the disease as a whole to the thalamus or more precisely its size (smaller is abnormal). The thalamus "seems to filter all information before it reaches the cortical areas of the brain." Nancy Andreasen of the University Iowa did the research, first reported in *Science* on October 10th, 1994.

19. During this season, at least China seems to get the last word in the competition with Africa for homelands or nesting areas of various taxa. It is apparent that from time to time fossil evidence, whether paleontological (in this case) or archeological, triumphs over distributional evidence. From the modern distributions of men, apes, monkeys, and their more primitive relatives like lemurs, tarsiers, aye ayes, bush babies, and the like it seems more likely that primates as a whole got their start in Africa. I don't know how their relationship to tree shrews bears on this. However, in more late-breaking news in

the *National Geographic Magazine*, vol. 186, no. 5, Geographica, November 1994, it said: "A tiny jaw found in a quarry at Shanghuang west of Shanghai adds weight to a revolutionary theory that higher primates first scrambled around Asia, not Africa. The jaw (...) belonged to a mouse-size primate (...) that lived during the Eocene 45 million years ago, long before monkeys, apes and humans evolved. Four other new primitive primates have also been excavated, including the earliest tarsier and a squirrel-like primate resembling one found in Wyoming. Mary Dawson and Christopher Beard of the Carnegie Museum of Natural History and Chinese colleagues made these finds and similar ones on the Yellow River in northern China, where they worked with support from the National Geographic Society."

"The diversity of early primates at Shanghuang is unprecedented in Asia and led one expert on primate fossils to dub the site 'a primate Garden of Eden'." Score one goal for China! By the way, the Carnegie Museum is in Pittsburgh, PA. The proper nomenclature for the tiny jaw is *Eosimias sinensis*.

20. One oddity to finish off Quick Notes on an even number. When marine biologists from Woods Hole (Massachusetts) got way down deep in the Pacific off the coast of South America, they saw two of the rare octopi. First time to see them at 8,000 feet below sea level. The two were intertwined in what looked clearly like intercourse. Closer inspection revealed that it was such, but that the two mates were (a) members of different species and (b) both males! It's dark down there! Nonsense, said my wife. Now we know that there are two species of gay octopus.

## ANNOUNCEMENT OF A SPECIAL REVIEW

In our next issue, *Mother Tongue* 24, we will devote a major portion of the issue, or all of it if necessary, to a tripartite review of *The History and Geography of Human Genes* by L. Luca Cavalli-Sforza, Paolo Menozzi, and Alberto Piazza. Princeton University Press, 1994. 1069 pages, 518 maps of gene distributions, 8 color plates, many tables and a roughly 1500 source bibliography. This book is Luca's Meisterwerk and the most comprehensive statement of gene distributions since Mourant's blood group studies of a generation ago and Arthur Steinberg's compilations of Gamma Globulin in the 1980s. It is also an attempt to write a big portion of human prehistory and to ally linguistic genetic taxa and their distributions with biogenetic distributions, cladograms, and measures of genetic distance. It is also a massive alternative to mitochondrial DNA conclusions, when and if they disagree, because the data that Cavalli-Sforza and his colleagues operate from are drawn from genes which are ultimately of *nuclear* DNA origin.

Since, as Becky Cann and I discovered above, the family trees of nuclear DNA origin (e.g., blood groups, serum proteins, etc.) are not constructed the same way that mtDNA ones are, these more traditional cladograms give information less available to mtDNA cladograms, precisely because the mtDNA research seeks to eliminate these traditional factors, most particularly *gene flow*. The traditional cladogram may tell you that the Ongota are a population much like the Tsamai but quite different in some particulars, yet the mtDNA results may say that the Ongota are simply Tsamai. The reason is that most Ongota men take Tsamai wives.

As an aid to understanding the importance of this huge book, we will have three people reviewing it. First, HF will review from the standpoints of Africa, paleo-linguistics, and the general view of the "emerging synthesis". Much of the basic information in the book will be given in precis form, but criticism or praise for specific prehistoric conclusions will be added. There are some for Africa. Secondly, Beck Cann will give a review framed more as a general comment and critical appraisal than a factual summary; this from the perspective of a founding mother of the mtDNA approach. Third, Frank Livingstone will review in the same terms as Becky, but his view is that of a somewhat more traditional physical anthropologist, interested in genetics but also natural selection and disease. Frank is less familiar to long rangers, even though he is a founding member of ASLIP. He is famous for his work on the sickle cell in Africa and genetic defenses against malaria elsewhere. Becky is at the University of Hawaii, while Frank is at the University of Michigan, where Milford Wolpoff is friend and colleague but not necessarily leader. (Later, Frank will also favor us with a longish commentary on the alternative hypotheses about modern human origins. He is not overly inclined to favor any of them.)

We conclude with a basic summary of the book, written by the editors and/or the public relations people at Princeton University Press. Actually, the latter are quite a



competent lot. We expect that everyone will know that this summary is a "book jacket" job, an advertisement in so many words.

That does not necessarily make it false, however. Knowing the quality of the audience, Princeton University Press has kept the summary pretty factual and close to the contents of the book. With that in mind, we offer it now.

(Quoting)

L. Luca Cavalli-Sforza and his collaborators Paolo Menozzi and Alberto Piazza have devoted fourteen years to one of the most compelling scientific projects of our time: the reconstruction of where human populations originated and the paths by which they spread throughout the world. In this volume, the culmination of their research, the authors explain their pioneering use of genetic data, which they integrate with insights from geography, ecology, archeology, physical anthropology, and linguistics to create the first full-scale account of human evolution as it occurred across all continents. This interdisciplinary approach enables them to address a wide range of issues that continue to incite debate: the timing of the first appearance of our species, the problem of African origins, including the significance of work recently done on mitochondrial DNA and the popular notion of an "African Eve", the controversy pertaining to the peopling of the Americas, and the reason for the presence of non-Indo-European languages — Basque, Finnish, and Hungarian — in Europe.

The authors reconstruct the history of our evolution by focusing on genetic divergence among human groups. Using genetic information accumulated over the last fifty years, they examined over 110 different inherited traits, such as blood types, HLA factors, proteins, and DNA markers, in over eighteen hundred, primarily aboriginal populations. By mapping the worldwide geographic distribution of the genes, the scientists are now able to chart migrations and, in exploring genetic distance, devise a clock by which to date evolutionary history; the longer two populations are separated, the greater their genetic distance should be. This volume highlights the authors' contributions to genetic geography, particularly their technique for making geographic maps of gene frequencies and their synthetic method of detecting ancient migrations, as for example, the migration of Neolithic farmers from the Middle East towards Europe, West Asia, and North Africa.

Beginning with an explanation of their major sources of data and concepts, the authors give an interdisciplinary account of human evolution at the world level. Chapters are then devoted to evolution on single continents and include analyses of genetic data and how these data relate to geographic, ecological, archeological, anthropological, and linguistic information. Compromising a wide range of

viewpoints, a vast store of new and recent information on genetics, and a generous supply of visual elements, including more than 500 geographic maps, this book is a unique source of facts and a catalyst for further debate and research.

(End of quoting)

For those wishing correspondence: Paolo Menozzi is Professor of Ecology at the University of Parma, Italy. Alberto Piazza is Professor of Human Genetics at the Medical School of Turin, Italy. Luca Cavalli-Sforza is Professor of Genetics at Stanford University, Medical School.

Two glowing blurbs by Colin Renfrew and Steven Jay Gould, which are on the book jacket, were not reproduced here, even though I agree with what they said, and *Mother Tongue* has published Gould's remarks a long time ago, when (by the way) they referred to Greenberg's work, not biogenetics.

Good luck, nos amis. The book costs \$150 some places and \$175 elsewhere. We thought to buy it for ourselves, but Princeton University Press gave us a copy.

# NOMINAL LEXICAL CATEGORIES IN EGYPTIAN

TAKÁCS GÁBOR

Székesfehérvár, Hungary

For my parents

Numerous traces of a former morphological distinction between different nominal semantic classes have already been pointed out in almost all branches of the Afrasian language family. This phenomenon has well-attested typological parallels in several other language families, e.g., Indo-European, Hurro-Urartean, Hattic, North Caucasian, Yeniseyan (Ket) (cf. Petráček 1988:97; Blažek 1983:243).

Thus, following his master N. V. Jušmanov, I. M. D'jakonov (Diakonoff) has elaborated his well-known theory on the existence of nominal postfixes (suffixes) marking different classes of animals in Proto-Semitic, viz., *\*-r* var. *\*-l* in the names of useful animals; *\*-b* was characteristic in the names of dangerous or harmful animals (Diakonoff 1970:461, note 23; 1967:210; 1965:28, note 40, 52, note 2; 1988:57; 1975:140). Similar results in other Afrasian branches also strengthened by D'jankonv's theory on Proto-Semitic *\*-r*/*\*-l* and *\*-b*, which met a wide acceptance in Afrasian (Hamito-Semitic) linguistics (cf. Franzaroli 1969:307, note 113; Illič-Svityč 1971:192-193; Kaye-Daniels 1992:443; Petráček 1988:97; Ehret 1989:199; Jungraithmayr's remarks on Diakonoff 1975).

The Proto-Afrasian feminine marker *\*(a)t* is also supposed to have been a nominal postfix of the category of passive objects previously (D'jakonov-Porxomovskij 1979:83).

Ehret (1989:109-110, 199) has identified the Cushitic parallel of Proto-Semitic *\*-b* "animate suffix" as *\*-b* "deverbative suffix forming nouns for parts of the body".

Still, there is Cushitic *\*-m*, *\*-u* in the names of dangerous, harmful animals, and *\*-r*, *\*-l* in the names of useful ones; in Chadic, *\*m-*, *\*u-* marked the semantic category of useful animals (cf. D'jakonov-Porxomovskij 1979:83).

Jungraithmayr (see his remarks on Diakonoff 1975) has observed Chadic *\*-l*, *\*-r*, *\*-n*, *\*-m*, *\*-k* in animals' names.

Chadic *\*-k* was also a postfix in the names of body parts (Skinner 1977:32, note 146).

The Chadic Sura prefix *kà-* marking the names of things woven of reed, baskets, etc. has been compared by Hodge (1966:52, 1971:42-43) to an independent lexeme in Egyptian, namely, *\*k* "basket woven of reed" (the word is reconstructible on the basis of the hieroglyphic sign for *k*; the word may have originally contained a weak consonant as well).

Proto-Semitic *\*-b* and *\*-l*/*\*-r* have only very few convincing parallels in Egyptian: Proto-Semitic *\*aj-r-* "ass-foal" ~ Egyptian *\*j/-z* "ass"; Proto-Semitic *\*naji-j-al-* "a sort of antelope" ~ Egyptian *nj-i-w* "ibex" ~ (Cushitic) Beɗawye *nā'i* "goat"; Proto-Semitic *\*ig-l-* "calf" (< "young antelope") ~ Egyptian *\*g-z*/*\*g-n* "calf" (for the reconstruction, see also Watson 1980:45); Proto-Semitic *\*imm-ar-* "ram" ~ Egyptian

*jm-z.t* "female ibex"; Proto-Semitic *\*di'-b-* "wolf" ~ Egyptian *z3-b* "jackal".

Beside these rare external parallels, so far no evidence gained by internal (Egyptian) comparison and reconstruction has been pointed out to show that the use of markers for different nominal semantic categories was characteristic also of the Egyptian branch of the Afrasian language family. Now, as one of the results of step-by-step biconsonantal reconstructions within Egyptian, the existence of three nominal postfixes of this kind can be postulated in Egyptian:

1. One of the several functions of the Egyptian postfix *-č* was to mark the semantic class of snakes and worms. Examples are: *hm-č* "designation of a snake" (Pyr., Wb II 491, 7), *hm-č.t* "id. (fem.)" (Pyr., Wb II 491, 8) (< *\*hm* onomatopoeic root [for the reconstruction, see also Conti 1980:54-56]), cf. *hm-hm.tj* "as a designation for vile beings, among others in the form of snakes" (Sp., Wb II 491, 4; act. "the roaring one" or "the hissing one" ?), *hm-hm.tj* "the roaring one, as a designation for lions" (Gr., Wb II 491, 3), *hm-hm.t* "cry, roar" (MK, Wb II 490), *hbm* (to be read *hm-hm* ?) "the hiss of a snake" (Gr., Wb II 502, 12), *hmj* "cry of exultation" (Gr., Wb II 490, 3), *hm* "a cry of pain" (NE, Wb II 490, 2), *hm-hm* "to roar, to bellow" (Gr., Wb II 491, 2), etc.; *fn-č* "snake, worm" (Pyr., Wb I 577, 5) (< *\*fn-* "to roll, to twist, to turn"), cf. *n-fn-fn* "of the treatment of woven material (?)" (Med., Wb II 252, 11; Conti 1980:54), *n-fn-f* (< *\*n-fn-fn*) "creeping, crawling ones, worms" (Sp., Wb II 252, 10); Westendorf 1977, 515, note 2: act. "rolling, twisting, turning"), *fn-č* (with rope-determinative) "winding (?)" (in the expression *fn-č-jb* "acute, sharp, intelligent [?]" < "of winding mind [?]", MK, Wb I 577, 1); *šn-č* "a snake" (Pyr., Wb IV 519, 2) (< *\*šn* "to roll, to twist, to turn"), cf. *šn* "loop of cord" (the word is reconstructible on the basis of the hieroglyphic sign for *šn*, Gardiner 1927:507, no. V7-8), *šn-j* "to encircle (originally with a rope)" (Pyr., FD 267), *šn-w* "cartouche" (originally "a loop formed by a double thickness of rope", Gardiner 1957:74; FD 268), *šn* "ring" (MK, Wb IV 488, 9), *šn-w* "rope, cord" (MK, Wb IV, 509). This postfix *-č* can be connected with an independent lexeme in Egyptian, namely, the monoradical root *\*čVč* ([?], the reconstruction of the weak component as *\*-u* is uncertain) "to bind" > "rope, snake", cf. *\*č* "rope with two loops" (the word is reconstructible on the basis of the hieroglyphic sign for *č*, Wb V 337, 3), *čč.t* "tie, binding" (Pyr., Thausing 1941:24), *čč-w* "a snake" (Pyr., Wb V 414, 2), *n-čč* "to fetter, to chain, to shackle" (Pyr., Wb II 367, 2), *n-čč* "fetter, chain, shackle" (Pyr., Wb II 367, 9), *jn-č.t* "fetter, chain, shackle" (Pyr., Wb I 102, 14), *jn-č* "to fetter, to chain, to shackle" (CT, FD 24), *n-kt* (dissimilation from *\*n-čt* < *\*n-čč*) "fetter, chain, shackle" (NK, Wb II 348, 3), (?) *b-čw* "designation of poisonous snakes" (NE Mag., Wb I 485, 11), *b-n-d* (*\*b-n-t* < *\*b-n-č*) "to wrap, to envelop" (NE, Wb I 465, 2), *b-b-n-č* "string, tape, band (?)" (Pyr., Wb I 455,

- 15; Thausing 1941:24), *s-čw* “as a name of a snake” (NK, Wb IV 357, 9). It is fairly interesting that, from both semantic and phonological points of view, Egyptian *-č* (< *\*-k* or *\*-ku* at the Proto-Afrasian level) “postfix marking the semantic class of snakes” and *\*čV<sub>u</sub>* “to bind” show a surprising similarity to the above-mentioned morphemes Egyptian *\*k* “basket woven of reed” ~ (Chadic) Sura *kà-* “postfix in the names for things woven of reed, baskets, etc.”.
2. Egyptian *-i/-r* (< *\*-r*) denotes the lexical category of internal organs of the body connected with breathing. The examples are: *zm-i* “lung” (act. “the blowing, booming one”; Pyr., Wb III 445, 15) (< Proto-Afrasian *\*zam-* “to blow, to play a wind instrument” [Bomhard 1990:348, 1989:44]), cf. Arabic *zm-zm* “to rumble, to roll; to murmur” (Calice 1936, 780.§), Egyptian *zb-i* (< *\*zm-i* ?) “to play a flute” (OK, Wb III 433, 4-6), *zm-i* “jubilation, joy” (act. “shouting in happiness”; Gr., Wb III 452, 1); *wf-i* “lung” (BD, Wb I 306, 3) (< *\*wf* “to blow”), cf. *\*f* (act. *\*wf* [?]) “horned viper” (act. “the hissing one” [?]; the word is reconstructible on the basis of the hieroglyphic sign for *f*, and the Demotic *ff* “viper”; Gardiner 1927:466, no. I9; Wb I, 571, 12), *wff* “a kind of snake” (Pyr., Wb I 306, 5). For the semantic shift of *\*f* and *wff*, cf. Arabic *fh* “to hiss (of snake)” (< Proto-Semitic *\*ph* “to breathe; Zaborski 1971:79, 169.§); *\*nf-r* “vent-pipe” (the word is reconstructible on the basis of the hieroglyphic sign of the phonetic value *nfr*; Wb II 252, 12; see also Wölfel 1955:44) (< *\*nf* “to blow, to breathe”, cf. *nf.t* and *nf* “breath, wind”; Med., FD 131), *nf.t* “fan” (OK, Wb II 250, 10), *nff* “to breathe out, to exhale” (NE, Wb II 250, 11), *nfi* “to sneeze out, to snort out” (CT, Wb II 252, 3), *fn-ğ* (*\*nf-ğ* ?) “nose” (OK, Wb I 577, 10-15) (< Proto-Afrasian *\*nVf* “to blow”; Majzel’ 1983:217-218; Bomhard 1984:204). Recently, the Czech Afrasianist Václav Blažek (1994:2) has suggested to me that the Egyptian postfix *-i/-r* may also be compared to independent Afrasian lexemes with monoradical structure: cf. Proto-Semitic *\*ir-at-* “breast” ~ (Berber) Tuareg *ta-rū-t* “lungs”, Šilḥ *t-ur-tt* “lobe of the lungs”, *t-ur-in* (pl.) “lungs” ~ East Cushitic *\*ir-/\*ur-*: Burji *ir-a* “stomach”, Somali *uur* “id.”, Rendille *ur* “id.”. This correspondence would indicate that the Egyptian postfix pointed out by me by means of internal Egyptian reconstructions is of Proto-Afrasian origin.
3. Another function of *-č* was to mark the semantic class of liquid materials. The examples are: *sf-č* “oil” (OK, Wb IV 118; FD 225) (< *\*sf* “to pour out, to flow”, cf. *sf-sf* “to pour water”; XXII., Wb IV 118, 1), *sf-r* “one of the anointing oils” (early OK, Wb IV 115, 1), *sf* “to mix (with liquids or granular substances)” (Med., Wb IV 114), *jr-č.t* “milk” (OK, Wb I 117) (< *\*iVr*, hence *\*iV’*, “milk”), cf. *j3.t* “milk-goddess” (Pyr., FD 7; Wb I 26, 16-17; Faulkner 1969, 131.§, note 2), *j3.t.t* “milk or cream” (OK, Wb I 27, 1; FD 7), *jr.tj* “milky” (Pyr., Wb I 116, 6), *jrj.t* “milk-

cow” (XVIII., Wb I 114, 18; FD 28). Perhaps the same *-č* occurs (as a root determinative) also in the verb *z3-č* “to sprinkle water” (Pyr., Wb III 422, 11) (< *\*z3/\*zr* “to flow”), cf. *z3-b* “to flow” (Pyr., Wb III 420, 3-4), *zwr* “to drink” (Pyr., FD 217), *zr-m.t* “a kind of beer” (MK, Wb III 463, 7).

As a conclusion, it can be proposed that morphological distinction between nominal lexical categories existed also in the Egyptian branch of the Afrasian language family. A few nominal postfixes in Egyptian seem to be of Common Afrasian origin. Nevertheless, the use of *-č*, *-i/-r*, and *-ğ* was characteristic of Egyptian only — the comparable cognate morphemes (if there are any) in the other branches did not have the function of a postfix. The limited number of examples for the three new Egyptian nominal postfixes seems to indicate that these postfixes become non-productive very early; possibly in the Old Kingdom or even at the “Proto-Egyptian” level (that is before 3,000 B.C.).

Abbreviations: BD = Book of the Dead; CT = Coffin Texts; Gr. = Greco-Roman Period; Mag. = Magical Texts; Med. = Medical Texts; MK = Middle Kingdom; NE = New Egyptian (Late Egyptian); NK = New Kingdom; OK = Old Kingdom; Pyr. = Pyramid Texts; Sp. = Late Period.

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## A MILD REJOINDER TO LYLE CAMPBELL

HAL FLEMING

Gloucester, Massachusetts, USA

I promise not to use any pejorative language or yell at anyone. My part in the endless game of thesis, antithesis, thesis, antithesis, th... will end after *Mother Tongue* 24. It seems a form of intellectual tennis, doesn't it? I want only to say four things herein. Well, a small fifth one as well. Campbell doesn't do outright *ad hominem* remarks, but the indirect zingers he slings are similar. For example, his remark about Fleming and Shevoroshkin being "mostly discounted boosters" is a wee bit on the *ad hominem* side. Of course, I've been punching him as he has been punching Greenberg. But he denies that he is punching anyone even subtly. Just doing his job telling the truth.

Well, a sixth one. I thought his article was an excellent and well-argued statement about the merits of his position and (allegedly) most other Americanists. In fact, he even made me feel guilty, although I'm not so sure why. Good arguing technique?

First, I propose a *synthesis*. Even non-Marxists can do that. Among other things, we can admit that some things are wrong with our own positions, we have weaknesses, and so do our opponents. We can concede some things. But also, we can recommend that our opponents grant us some strengths, while admitting gladly to our weaknesses. Our greatest weakness, it seems to me, is our failure to admit that two generations of textbook writers and university lecturers have established an Indo-European style of historical linguistics in the minds of almost all younger historical linguists. We just can't get around that. They believe their training, their investments in the field, and they just will not believe that Greenberg et al. style historical linguistics could possibly be right, even if Greenberg had successfully classified the whole world.

This is the essential wisdom of the Muscovite school. To paraphrase an older remark by Militarëv: "We have to play by Indo-European rules or otherwise they will slaughter us!" At least they have to *say* that they play by Indo-European rules. Then, when they propose phyletic connections by basically Swadesh/Greenberg rules, they cannot be attacked for having violated *Orthodoxy*. Our good coeditor, Bomhard, plays by the same rules.

Mind you, I do not believe in the ultimate superiority of the Indo-European mode of reasoning. I am impressed more by the self-applauding style of Indo-Europeanists and their Americanist students. But I have said that many times, and you all are tired of hearing it.

Our greatest strength, us long rangers, is hypothesis breeding or creation. Next to that is our better understanding of what science is all about. Historical linguistics is *not* a branch of mathematics or formal logic. As Greenberg has argued brilliantly in an unpublished paper, the concept of *proof* is

misused by linguists — from a scientific standpoint. Indeed Campbell's eloquent paper stands *au fond* on the mathematical proposition that your idea cannot be true if you have not proved it formally. But that ain't science, baby! Charles Darwin's theory has *never* been *proven*, has it? It just gets less and less likely to be false or more and more likely to be true. "Proof" is for algebra or courts of justice. Math aids science, but is not itself a science.

But math can confuse science. Example: Ringe has proven mathematically that "(...chance can explain Greenberg's 'evidence')", while Ruhlen has just proven mathematically that chance cannot explain such evidence (in his new book *On the Origin of Languages* [1994], pp. 279-283). Are they both right? Or wrong? Does it matter?

Nevertheless, the simplest truth of all is that most historical linguists believe that this kind of pseudo-mathematical orientation is what their science is about and ought to be about. They cannot be dissuaded. So be it. We will have to quit trying to get them to do otherwise. *Simplement dit, c'est fini.*

Second, I suggest we discard the notion of splitters and lumpers. I doubt the utility of that dichotomy. How about taking a more maritime analogy. Since I live in the town with the statue of "Captains Courageous" featured downtown — Gloucester, Massachusetts —, I wonder about the difference between lobstermen and whalers. Lobstermen are brave, decent, law-abiding and hard-working, but they stick near shore. Whalers may be a murderous lot, by contemporary standards, but they would sail far out on the grey and threatening North Atlantic and indeed sail around the terrifying Cape Horn, just to satisfy their lust to kill majestic whales for money. Not an admirable lot, but venturesome. Still better is the image of the "coasters", the sailors who traveled cautiously around the Mediterranean and more bravely round the shores of the Indian Ocean along the spice routes. The "coasters" contrast with the ancestral Polynesians who sailed clear across the mighty Pacific, often by dead reckoning or following bird flights, just to find small islands. Incredible, really! Naturally, I propose that long rangers be thought of as scientific Polynesians and their opponents as lobstermen, albeit punctilious, high-tech lobstermen.

Third, I have to correct the record. What Campbell says about me and some other Africanists in relation to African taxonomy takes many liberties with the truth, unfortunately. The whole African section of his argument is indeed distorted. (See my recent reply to Poser for some perspectives on Africa.) More specifically, both Sally Thomason and Lyle have been trying to catch me saying that I did not agree with the Khoisan phylum (hypothesis). But, heck, I know what I said and so do many other Africanists. For the purposes of one paper, I said we should adopt the famous "null hypothesis" and start all over again with Khoisan, *as if* we did not believe it were valid. But I ended up that venture a bit later with a paper on Khoisan itself in which I was quite satisfied with the validity of Khoisan. Professor Westphal always opposed Khoisan, but his influence



has waned considerably. Guthrie opposed Bantu as Niger-Congo, but who would agree with him nowadays? The exact status of Mande, Kordofanian, and also West Atlantic as subphyla of Niger-Congo have been discussed by many workers in the field, none of whom have abandoned the overall taxon of Niger-Congo (née Niger-Kordofanian). They are reconstructing it now, not rejecting it. Lyle's information is way out of date.

Furthermore, most workers have agreed that Nilo-Saharan is more troublesome to work with, but also much less worked on, than the other major African phyla. Bernd Heine is quoted twice as saying that Nilo-Saharan has not been proven yet and seems pretty weak. The reader will note that he said exactly the same thing in 1972 and 1992! This suggests that Bernd didn't give the matter much thought in 20 years. Please note that he was not saying that Nilo-Saharan was wrong or invalid; he was mostly cluck clucking about procedural improprieties. He meant that its reconstruction had not been done and that its internal structure had not been agreed on yet. A pretty good working hypothesis, a respectable one, but one where some tinkering was required and much more field work was needed. Why do Heine's remarks — and Bernd is a proper conservative Prussian — why do they show how wrong Joe Greenberg is or was?

Besides, even Bender, who now throws darts at Greenberg and spears at me, has devoted the last 15 years of his life to the purportedly invalid taxon of Nilo-Saharan. Sure he has changed the internal structure — and so has Christopher Ehret —, but why does that make the overall taxon wrong? Would Lyle suppose that Indo-European was invalid because Indo-Europeanists cannot agree on the placing of Anatolian within Indo-European or whether it really is split into *satem* and *centum* or not, or whether Baltic cum Slavic or Italic cum Celtic are special units or not?

Campbell's remarks about Cushitic and Omotic are simply wrong. I can understand his motivation, but this time *he* is the one who obviously did not consult the specialists before making his comments. All the changes and proposals for change in the Afroasiatic realm are changes of sub-classification. Does Lyle believe that taxa are set in concrete from the moment of their creation? Perhaps as a mathematician he does. Yet as a scientific construct, an hypothesis about prehistory, Afroasiatic changes over time. New data come in. New insights occur to people. People fight about details and curse the agents of change. But the overall taxon — Afroasiatic — is in fine shape. Is the Greenberg classification of 1948-1963 wrong because people in the '70s and '80s re-adjusted the internal taxonomy of it? Let us not be ridiculous! Campbell's whole argument is pervaded by a horror of error. People should not make mistakes in detail and, if they do, their general hypotheses must be discarded. That is mathematical type thinking, nicht wahr?

By the way: both at the Ethiopianist conference last month and elsewhere, Bender has applied the "Ringe test" to Nilo-Saharan and found everything okay. How could he work on proto-Nilo-Saharan, which he is deeply involved in, if he allowed the "Ringe test" to show that Nilo-Saharan was not too promising? Campbell did not get the Mao business straight

either. Italian "authorities" had assured Greenberg that Mao was just part of Koman; there was not too much published field data to test that notion against. Bender later got some field data on Mao and added it to his Komuz group. It was I, not Bender, who took Mao out of Koman and put it into Omotic. Recently, Bender has decided that Mao had flunked the "Ringe test". So sad, because it is now a *solid* part of North Omotic. You know, sound correspondences and that sort of thing.

As a postscript to the African question, please allow me to borrow a statement by an Englishman, Colin Flight, about Greenberg and his British critics. They were his fiercest critics, even if Semiticists fought the new Afroasiatic in very nasty ways. Ruhlen in his other new book *The Origin of Language* (1994) quotes Flight as saying:

... rather than trying to understand what Greenberg had to say, British linguists seem to have done their best to *misunderstand*. They acted as if they were determined to miss the point — as if it was their business to spot passages in the text which were loosely worded, ambiguous, or elliptical, and then to place upon these passages the most inept construction that could possibly be devised. In a word, they acted like lawyers (Flight 1988:266).

Aye, that's a terrible thing to be accused of, acting like a lawyer!

Nevertheless, it is not a very convincing argument to say that Amerind must be a valid hypothesis because Nilo-Saharan and the other African phyla are valid; all created by Greenbergian genius. There is a weak logic to it, my friends, even if we have all used it at one time or another. We need to look at the Amerind hypothesis as if it had been invented by Pierre Alexandre, whose pronouncements carry slight weight outside of France. If the opposition considers the evidence itself, and not their apparent obsession with a satanic Greenberg, then maybe the Amerind hypothesis will get more reasonable treatment. Let us declare that someone "worthy of respect", like Eric Hamp, had written almost exactly the same book, but in the year 2008. How would it be treated, I wonder?

One would hope that the overall taxon would be evaluated in itself, in its entirety, instead of dozens of specialists working to prove that their details do not fit the general scheme. Any specialist can find fault with a general hypothesis, often tellingly so, if s/he is *motivated* to knock it down. Many of us do not believe, Professor Campbell, that your multitude of specialists was motivated to make fair or objective criticisms. And are your experts on Iroquois or Mayan any more qualified to make judgements about South American languages than our experts on Africa are? Do each of your experts survey *hundreds* of native American languages before deciding that the man who *did survey* those languages must be wrong? I try not to pass myself off as an expert on Niger-Congo's hundreds of languages, even if I have worked on a few Bantu tongues. Aren't your people a bit presumptuous and a tad dishonest?

Fourth, we have now heard it officially (from Lyle Campbell). The Americas, two linked continents settled

between 10,000 and 30,000 years ago, have 145 linguistic phyla in them. Good grief! Now the mills of the gods do grind slowly and, yea, they do grind exceedingly small. Truly. But when will we get some kind of useful prehistory out of 145 unrelated, well but maybe related, phyla? If I were an archeologist or biogeneticist, I would just write off the Americas as places where one could cooperate meaningfully with historical linguists. You guys will just have to do all the prehistory by yourselves, because you will get no help beyond the obvious relationships of a few millennia or so. The lobstermen prevail along these shores, and whaling is forbidden. Not properly methodological, you see!

As far as the specifics of the Amerind debate are concerned, as usual I leave it to Greenberg or Ruhlen to respond. We will publish the rebuttal. Now that we have given Campbell a fair space to have his say, I wonder if *Language* or *IJAL* will let Ruhlen say a word or two about these matters in their staid pages? After all *Language* allowed Matisoff to thump Greenberg, but Matisoff is no more qualified to judge the Amerind hypothesis than I am. In fact, maybe less so, since he thinks you cannot relate languages more than 6000 years apart. So by the "Matisoff test", there could not possibly be an Amerind taxon. Once again, theory tramples on the data, and ideology triumphs over prehistory.

(You see, this game of tennis encourages volleys!)

Note: Merritt Ruhlen's first new book is published by Stanford University Press. The second new book is published by John Wiley & Sons, New York & Chichester & Brisbane & Toronto & Singapore. They are both powerful books; Merritt argues as compellingly as Lyle does.

See also Colin Flight, 1988. "The Bantu Expansion and the SOAS Network," *History in Africa* 15:261-301. Merritt codes it as "a look at the Bantu controversy from inside of SOAS." (That is, School of Oriental and African Studies, University of London, for those who don't know it.)

## PLUS ÇA CHANGE, PLUS C'EST LA MÊME CHOSE

MERRITT RUHLEN  
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Some etymologies are so widespread that their validity can hardly be doubted. An example of this type is the Amerind root *\*makan* "hand", which has reflexes in numerous indigenous languages of North and South America (e.g., Totonac *makan* in North America and Caripuna *moken* in South America). Other etymologies, because of their limited distribution or semantic variability, remain possibilities, rather than certainties. Such an example is the Nostratic root *\*sam-/səm-* "to resemble, to be like" posited by Bomhard and Kerns (1994:358-359). Their etymology is based on forms in only two branches of Nostratic — Indo-European and Afro-Asiatic —, and the Afro-Asiatic evidence is only found in two branches of the family, Semitic and Ancient Egyptian.

The Indo-European etymology is firmly established, and the traditional reconstruction is Proto-Indo-European *\*sem-* "one, together, like, same", with reflexes such as Tocharian B *seme* "one", *sām* "equal", Classical Greek *heis* (< *\*sems*) "one", Sanskrit *samā-h* "same, equal", Latin *similis* "like, similar", Gothic *sama* "same", and English *same*. For Afro-Asiatic, Bomhard and Kerns cited Ancient Egyptian *sm* "to resemble", *smt* "form, likeness", and Proto-Semitic *\*sam-al-* "to resemble", whence Hebrew *semel* "image, statue", Phoenician *sml* "image, statue", and, with metathesis, such Ethiopic forms as Geez *masala* "to be like", Tigrinya *mäsälä* "to be like", Tigre *mäsälä* "to be similar", and Amharic *mässälä* "to be like".

The etymological connection between these two families is not implausible; both the semantics and the phonetics are straightforward. Yet the distribution within Afro-Asiatic is restricted to the Semitic and Ancient Egyptian branches. Were this all the evidence available, one might never be certain that the putative etymological connection was historically correct. In this particular case, however, there is additional evidence that strongly supports — perhaps even confirms — the validity of Bomhard and Kerns' conjecture.

The first piece of evidence comes from Ainu, a language that Greenberg includes in his Eurasiatic family, but which Bomhard and Kerns exclude from their version of Nostratic — at least for the moment. The relevant Ainu form is *sem* "as, like, the same", reported in Batchelor's dictionary (Batchelor 1905:393).

Supporting evidence is also found in Amerind. The late Wick Miller (1967:68) reconstructed Proto-Uto-Aztecan *\*seme* "one" to account for forms such as Mono *simi* "one", Comanche *simi*? "one", Aztec *seemeh* "one", and Pipil *səm* "one", *səm-pual* "20" (literally, "one-counted [person]"). In the Penutian branch of Amerind, we find Sahaptin *sim* "only", Nez Perce *šim* "only", and Zuni *samma* "alone". In the Hokan branch, examples include Washo *šemu* "the one", Shasta *tsammu* "one", Chumush *čumu* ~ *šumu* "one", and Comecrudo

*somi* "alone". In the Paezan branch, I found two examples, Atacama *sema* "one" and Timucua *isimi* "only, sole, alone". Though members of the same branch of Amerind, it is noteworthy that Atacama was spoken in northern Chile, while Timucua was spoken in northern Florida. Both languages are now extinct. The final examples occur in the Ge family, where we find forms such as Chavante *simisi* "one" and Cherente *semiši* "one".

It is also possible that Bella Coola *smaw* "one" contains the root in question. Bella Coola is the most divergent Salish language and its word for "one" differs from those found elsewhere in the family (Ruhlen 1995). Furthermore, there is a Salish numeral suffix, *-aw*, that is attached to various numerals, so that the Bella Coola form is presumably to be analyzed as *sm-aw*. The first portion of this numeral may be the root under discussion.

Though this particular root is not omnipresent, as some other roots are, I believe there is a good chance that the Afro-Asiatic, Indo-European, Ainu, and Amerind forms are all cognate. That such strict semantic and phonetic similarities could have arisen independently — the only other explanation — in Africa, Europe, Asia, and North and South America seems to me unlikely. Furthermore, I have recently proposed some 60 etymologies connecting just the groups involved in the present case: Afro-Asiatic, Eurasiatic, and Amerind (Ruhlen 1994). Given this background, it would seem that we have here an exemplification of the title of this paper: The more things change, the more they stay the same [sem].

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## "MACRO-AUSTRALIC"

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What follows is not intended to be a finished treatise: it is merely a squib intended to stimulate thought and research about the possibility of a "super-mega-phylum" named, for now, "Macro-Australic".

"Macro-Australic" is not a new idea. Trombetti posited a macrofamily embracing Andamanese, Papuan, and Australian. Later, Swadesh proposed a "Macro-Australian", one of twelve world macrophyla in his (1960) *Tras la Huella Lingüística de la Prehistoria*, and essentially similar to the family proposed here. Václav Blažek reminds us that a similar idea was proffered by "P. Rivet (inclusive Ainu [and also Sumerian !] and without any methodology)".

This model is more inclusive than any of the earlier proposals. It embraces the Australian family, the Indo-Pacific family (per Greenberg), and Austric (Austroasiatic, Austronesian, Kadai, Miao-Yao, and *Nahali* and *Ainu*). An exhaustive lexical search has not been made yet: the list reflects sources available to the writer. As a beginning, the following comparisons are suggested:

- HAIR (1): **Indo-Pacific:** Trans-New Guinea: Kumukio *dzomor*, Selepet *somot*, Yagay *rumb*, Komutu *rom*, Nek *dom*, etc.  
**Austric:** Austronesian *\*d'ambut* "hair, head";  
Kadai: Laqua *dam* "hair, head"; Kelao *\*sa[m]*; Li *\*nom* "head hair";  
Miao-Yao: Proto-Miao-Yao *\*syám* "beard";  
Austroasiatic: Katuic *\*num* "to tie hair";  
Ainu *numa* "hair".
- HAIR (2): **Indo-Pacific:** Tasmanian (SE) *\*pöle* "hair (of head)"; (NE, ME, SE) *\*pūirí(na)*, *\*pöferi(na)* "hair, feathers, fur, scales".  
**Australian:** Pama-Nyungan *\*pula* "feather, hair".  
**Austric:** Austronesian *\*bulu* "down, hair, feather";  
Kadai: Tai *\*pru* "hairy".
- EYE (1): **Indo-Pacific:** Tasmanian (ME) *\*monje(na)* = *\*mōte(na)*;  
**Australian:** Common Australian *\*miri(ŋ)* ~ *\*mili(ŋ)*: Woiwurrung *mirriŋ*; Proto-Paman *\*maari*, etc.  
**Austric:** Austronesian *\*maCa* (? from *\*matra* or *\*mapra* ?);  
Miao-Yao: Miao *\*maay*;  
Austroasiatic: Munda: Kharia *məḍ* ~ *mōḍ*, Juang *e-mor*, etc.; Mon-Khmer *\*mat*;

- EYE (2):** **Indo-Pacific:** Tasmanian (SE) \**nūb(ě)-(rē-na)*; (N) \**námě-(riga)*; Torricelli: Gresik *nam*, Kemtuk *numu*, Wembi *nof*, Arapesh *nabe(p)*, Yambes *nambep*, Kavu *nambas*; Trans-New Guinea: Konda *nuburu*, Waris *nop*, Manem *nof*, Senggi *now*, Morwap *naf*, Usku *nifi*; **Australian:** Tiwi (archaic) *tupurra*; Nunggubuyu *munbarg* (dissimilated from \**nu(n)PVr-* ?); **Austrian:** (?) Ainu *nu* “eye” (in compounds).
- TONGUE (1):** **Indo-Pacific:** East Papuan: Reef *nælibi*, Banua *nalap*, Nea *nalepu*, Savo *lapi*, etc. **Australian:** Proto-Paman \**ñapil*. **Austrian:** Miao-Yao \**mbret* ~ \**mblet*; Austroasiatic: Bahnaric \**[r/l]əpiet*.
- TONGUE (2):** **Indo-Pacific:** Tasmanian (W) \**tólā(ná)*; **Australian:** Proto-Australian \**DHalanNY*: Common Australian \**dalan* = \**jalany*. **Austrian:** Austronesian \**dila?* ~ \**dilap* (contaminated with TONGUE [1] ?).
- MOUTH:** **Indo-Pacific:** Tasmanian (NE) \**mona* ~ \**mūn* ~ (ME) \**āmuna* ~ (SE) \**moyē* “lips, mouth”. **Australian:** Karnic \**ma(r)na* “mouth”; Yittha *mu(r)n*; Gudadj *munu*; Yuin-Kuri *mun*; Yaroinga *mena* “mouth”; Pintupi, Mirniny *muni* “lip”. **Austrian:** Austroasiatic: Chrau *mu’nh* “mouth”.
- LIP:** **Indo-Pacific:** Trans-New Guinea: Neme, Morawa, Binahari, Monomor *bebe* “lip”, Marani *bebe* “mouth”, Orai-iu *bebe-u* “mouth”. **Australian:** (?) Guwa *bewi* “mouth”. **Austrian:** Austronesian \**bibiR* “lips”; Kadai: Tai \**bi* “lips”; Austroasiatic: Proto-Mon-Khmer \**-pir* “lip”; Bahnaric \**?bir* “mouth, edge”; Viet *moi* “lip”.
- BLOOD:** **Indo-Pacific:** East Papuan: Nasioi *ereng*, Siwa *iri* “blood”; Trans-New Guinea: A’e *uring* “red”, Bogadjim *leng* “red”; Yega *ororo* “blood”, Namaui *aro*, etc. **Austrian:** Austronesian \**iRag* “deep red”; Formosan (Ami-Bunum) \**(qa)iyag* “blood”; Kadai: Sek \**riig* “red”; Mak *lap*, Li \**hleen*; Miao-Yao: Proto-Miao \**?liq* “red”.
- BONE (1):** **Indo-Pacific:** Tasmanian (ME) \**tola-māna*
- “rib”; (SE) *TOODNA* “bone”, \**taróna* “backbone”; (N) \**tiwan-* (rik) “bone”. **Austrian:** Austronesian \**tulag* “bone”; Kadai: Kam-Sui \**tlaak*; Tai \**?duuk* “bone”.
- BONE (2):** **Indo-Pacific:** Tasmanian (NE) \**pēnā-*, (ME) \**piñā-*. **Australian:** Marowa *bi(r)na*, *brinna*; Ulaolinya-Wonkadjera, Kana *binna*; East Victoria *bi’rin*. **Austrian:** Austronesian (Formosan) \**bani*.
- ARM:** **Indo-Pacific:** Andamanese: Puchikwar, Juwoi, Kol *ben* “shoulder blade”; Trans-New Guinea: Turama *bena* “shoulder”, Eme-eme *beno* “shoulder”, Goaribari *bena* “upper arm”; Asmat, Kajakaja *ban* “hand”, etc. **Austrian:** Kadai: Laqua *pag* “arm”; Lati *peñ* “shoulder”; Tai \**vian* “hand”; Miao-Yao: Proto-Miao \**mpag* “hand, arm”; Austroasiatic: Chrau *po’niq* “shoulder”, *po’nar* “wing”.
- FINGERNAIL:** **Indo-Pacific:** Tasmanian (W) \**péra*, (SE) \**pere*, (ME) \**peyéra(na)* “toenails”. **Austrian:** SW Pama-Nyungan \**piri*; Banggarla *birri*; Luridja *perri* “fingernail”.
- FOOT:** **Indo-Pacific:** (?) Trans-New Guinea: Kati *yon* “foot, leg”, Ningerum *don*, Telefol *yaan*, etc. **Austrian:** Common Australian \**jina(ŋ)* = \**dyina(ŋ)* “foot”: Tangkic: Minkin *tvag(k)a* “foot” (recorded as *CHANGA*), *tvag(k)ay* ~ *tvāNa* “track of foot”; Nyangumarta *jina* “foot”, etc. **Austrian:** Austroasiatic: Munda \**janga* “foot”, Mon-Khmer \**ḡyŋ*, etc.
- WOMAN:** **Indo-Pacific:** Trans-New Guinea: Banak *pana*, *fana* “woman, wife”; Dem *pani* “female”; West Papuan: Aitinjo *finya* “women”; Mogetemin *fanya* “woman”, etc. **Austrian:** Proto-Pama-Nyungan \**pamyji.l* “woman, female”; Guwa *buña(na)* “woman”; Bundyil *buño*; Karuwali *punja*, *punga* “woman”. **Austrian:** Austronesian \**binay* ~ \**binəy* “woman” (Malay *bini* “wife”); Proto-Polynesian \**fine* ~ \**ma/fine* “woman” (Hawaiian *wahine*, etc.).
- MAN:** **Indo-Pacific:** East Papuan: Taulil *loka* “man”, Siwai *lugang* “man”; West Papuan: Galela *roka* “husband”, Isam *lokat*

“husband”; Andamanese: Biada *liga* “boy”.  
**Austic:** Austronesian \**laki* “man (male), married man” (Tagalog *la/laki*);  
 Kadai: Lakkia *lak* “man (male)”; Tai \**laaŋ* “grandchild, child, young man”; Proto-Kam-Sui \**la:k* “person, child”;  
 Miao-Yao: Proto-Yao \**laaŋ* “son-in-law, young man”.

**ASHES:** **Indo-Pacific:** Andamanese: Beada *bug*, Bale *buk*; West Papuan: Asli-Sidi *bok*, Maibrat *buh*; Trans-New Guinea: Boazi *pokok*, Konmak *pokak*, etc.  
**Austic:** Austronesian \**ʔabuk* “dust”, \**ʔabuʔ* “ashes”; Proto-Polynesian \**efu* “dust”;  
 Kadai: Tai \**ʔbuk* “soft, friable” = “powdery”; Proto-Kam-Sui \**phwu:k* “ashes”;  
 Austroasiatic: Bahnaric \**bu:h* “ashes, dust”.

**FIRE:** **Indo-Pacific:** Tasmanian (ME) \**ḡēna*, (SE) \*(*ḡ*)/*una*, (W) \*(*w*)/*ūna*, NE \**ūna*, (N) \**unī*.  
**Australian:** (?) Woiwurrung *wiiny*;  
**Austic:** Austroasiatic: Sedang *ón*; Bahnaric \**ʔuñ*, Katuic \**ʔu:ʃh* “fire”; Khmer *ʔuh* “firewood”;  
 Ainu: *una* ~ *uina* “ashes”; *unci* (from \**un-ti*) “fire”.

The etymologies given here would have to reflect a very old proto-language (50,000 years, according to Hal Fleming). Note that all the comparisons involve basic vocabulary: all but “arm” pertain to the 100-word list for glottochronology, and four of the meanings (“eye, tongue, blood, fingernail”) are on the Dolgopolsky roster for stability.

Kruskal, Dyen, and Black note that “eye” (.11) and “tongue” (.17) are particularly stable (within the top five), and it is precisely here that we find multiple roots. Note also “bone” (.76), showing multiple roots, which is 24th on Kruskal, Dyen, and Black’s index. The very nature of these comparisons minimizes the probability of borrowing or diffusion.

Discussing an earlier version of this paper, Václav Blažek had these comments:

It is certainly possible, but there are also alternative explanations of Austic - Australian - Indo-Pacific coincidences:

- (i) later AN [Austronesian] diffusion in Australia (Blust) and N. Guinea (Wurm)
- (ii) Australian/Indo-Pacific substratum in AN (and Austro-Asiatic too ?)
- (iii) the heritage from proto-human.

Some of the resemblances here may possibly be attributed to diffusion (mainly Austronesian influence, e.g., on Papuan languages), but this could hardly be the case with Tasmanian, which was isolated from the rest of the world for 10,000 years, until 1642 (cf. Jared Diamond, “Ten Thousand Years of Solitude”, *Discover*, March, 1993). Tasmanian words, as reconstructed by Wilhelm Schmidt, figure in nine of the eighteen comparisons. Clearly, the Tasmanian languages (however tattered the remnants) are invaluable for the understanding of deep relationships in the region.

A heurism for this proposal is found in Cavalli-Sforza’s genetic classification of human lineages, in which virtually all the major populations speaking Australian, Indo-Pacific, and Austic languages make up the vast “Southeast Asia” branch. (The only anomaly is the South Chinese population: this likely represents replacement of Austic languages by Chinese, and the area is still mottled with Austic minorities.) Also, on Cavalli-Sforza’s tree, the Ainu (though Austic in my model) are grouped with the North Eurasian branch. On the other hand, Christy Turner’s dental studies show that the Ainu cluster with the Sundadonts (Southeast Asia, Indonesia, Polynesia) rather than with the Sinodonts (North China, Mongolia, Siberia, Native America).



## ASLIP BUSINESS

We have just two items of business to consider. First, a trivial matter. Second, a very heavy one.

*Gifts solicited.* When we began *Mother Tongue* just eight years ago, all we had to print out our incredible proposals was a basic Epson printer. It wore out. In 1990, Allan Bomhard gave me a Tandy printer to use for *Mother Tongue* purposes. Ultimately, due to the set-up of my office and the increasing age of the Tandy, I snapped up a bargain sale of a new Hewlett Packard Deskjet 520 (plus a cable, etc.) for just about US \$300. It works beautifully. It could legitimately be written off as a cost of doing business (re the income tax) for me or ASLIP, except that I am not eligible (retired) and ASLIP is too poor to be taxed. So it comes out of my own pocket.

I would like to share this expense with you.

My, isn't that brazen! you might say. However, it could reasonably be paid for by ASLIP, but that would be the cost of most of an issue of *Mother Tongue*. I don't want to do that. Like many retired professors, I am less than rich. Like most of them, I have had a reduction of income because pension plans tied to the stock market have paid little or nothing this year. And those were the good investments. Anyway it is hard for me to pay for it.

So any contributions, large or small, will be appreciated!

### THE BAD NEWS FIRST

#### MATRICIDE

We are planning to kill our mother. However, like good Jains we know she will be reincarnated. Ah, but in another form. Who are the we? The editorial we, but this time a dual.

What concerns ASLIPers the most is that everybody's membership expires on receipt of the last issue — *Mother Tongue* 24 — which should reach you around the middle of January, possibly earlier. For those who have already paid their dues for 1995, their money will be refunded. For our special life time member, of course, there will be no cancellation. Whatever we do after January, she will be a part of it. For those who qualify for the re-incarnation and have paid their dues for 1995 (effectively nobody), they can apply that money towards their new dues.

*Mother Tongue* is going out of business as an ostensible journal.

Despite the flood of crocodilian tears at the University of Pittsburgh, Harvard, Berkeley, and the Smithsonian, we are determined to do it.

What are the reasons? Oddly enough, it is not a drop in membership; that is actually growing. Nor is it a belated

realization of the superior cogency of arguments against long range comparison. We see more clearly now that the opposition embodies ideology and social conformity. They stopped doing empirical science a few years back. Even though their main strategy has always been to shout down long rangers — that includes pressures to conform within departments —, their glacier of conformity is melting around the edges. So it seems from our steady trickle of new members. Yes, even the fine new mathematical models which have the internet freaks all abuzz do not drive us from the field. The Ringe model is silly and has already been shot down at one conference. Several papers refuting Ringe's views are in the pipe line. One member already knows enough to sink the Bender-Ringe ship. But seriously inhibiting her is her fear of controversy.

So we are gaining members, and we will win the argument, whenever "official" linguistics journals in the United States come to accept freedom of speech as a necessary part of science. Long range hypotheses may be stomped out, just as Sapir's, Kroeber's, and Swadesh's were! Yet sooner or later, they must prevail because they reflect important truths about prehistory. By the way, we refer here to so-called mainstream linguistics. In the freer realms of fringe or minority linguistics — sometimes found only in the libraries —, good ideas lie dormant waiting for a generation of freer scientific spirits to take back the mainstream.

We are *not beaten*. We decide freely to struggle no more with hyper-cautious linguists because we have drifted too far from our original course. Hal did not begin *Mother Tongue* in order to spend eight years debating with silly people, though he suspected that such might happen. We all started out to get some exciting research done via international cooperation and to tell the world about the bold ventures of the young Muscovite historical linguists. Go back and look at *Mother Tongue* 1 and *Mother Tongue* 2 to get the full flavor and excitement.

But we *are tired*. Producing our little quasi-journal takes an unwarranted amount of time. For eight years, we two have done just about all the work, except for Ekkehard Wolff's kind help. (Mark Kaiser did one issue.) We pumped out a lot of information and opinion on the "emerging synthesis". Still, being an editor is a thankless job, said a snide Terrence Kaufman some years ago. Oh yes, alas, amen! Hypotheses which we introduced first in this newsletter have gone into the literature as other people's ideas. Members write books and articles but never mention *Mother Tongue*. Hal for one is quite sick of it.

We have also been struck by something more serious. Many members seem to read little, or at least to remember little, of particular issues. One told me he never read anything except ASLIP business; another only the archeology parts. Another could not remember *Mother Tongue*'s name — this was a founding member. Many in their own publications show that we have had zero impact on their thinking. Etc. What a lousy substitute for long range research all this publishing is!

## NEXT THE GOOD NEWS

## REINCARNATION

The spirit of an old-fashioned German university seminar more than anything else represents the new focus on ASLIP, rather than *Mother Tongue*, the newsletter. While we have not yet thought through the whole operating system, we can announce some principles now. Members may comment on these as they see fit. Eventually, the Board of Directors will have to rule on the proposed changes, although few of our By Laws will be affected.

1. ASLIP exists primarily to do and to foster research.
2. Two latent functions, specified in the By Laws, will be stressed more in the future, namely the creation of a data bank and the acquisition of funding for said data bank and for supporting specific targeted research.

(2a) Several current long rangers have been building large data bases for some years now, including Mary Key, Gene Gragg, and Vaclav Blažek. Many other long rangers have impressive data banks.

(2b) Support for specific research need not be limited to linguistics; some highly significant biogenetic and archeological research in key areas (e.g., non-Arab Sudan, western Ethiopia, Oman, etc.) ought to be supported.

3. The encouragement of long range hypotheses of substance, rather than hyper-caution, will be the style. In the nature of things, these are likely to be linguistic. However, such ancillary theories as homelands and time depths will involve archeology and/or biogenetics to a certain extent.

(3a) Extra strong encouragement of research in homelands theory and linguistic dating is proposed. Those are seen as vital matters for prehistory, even if not for linguistic taxonomy. There is considerable confusion about these matters at the present time, and a fair amount of nonsense is written about them. Few people are "trained" in these topics, which are remarkably neglected considering their importance and how fascinating they are.

4. The active sharing of data, concepts, and theories within the confines of ASLIP, rather than trying to publish them in *Mother Tongue* on a regular basis, is a vital goal of ours. Informal networks for doing such already exist among our members. Our intent here is to bolster their researches, disseminate them to *interested* colleagues, test theories informally, and eventually publish the results in *Mother Tongue*. Much of our new \*MT Treatment, borrowed from *Current Anthropology*, is applicable here.

(4a) The protection of hard-working researchers from

plagiarism is a secondary goal. People should get credit where it is due them, especially in publications.

- (4b) The development of an internationally valid system for writing *phonetics* is to be stressed. Such systems as IPA are clearly obsolete and not used by many. Yet the IPA as a grand attempt and a comprehensive one should be a practical starting point. A great deal is known about phonetics and phonology in linguistics. Our problem is to get the Russians and Americans, Indologists and Semiticists, Africanists and Southeast Asianists to agree on some conventions, like most mature sciences have. For example, just what does [c] or [j] mean? Or why use such difficult symbols as [ʃ] when most typewriters and computers can easily do [ʃ] or Czech's marvelous set of [š, č, ž]?

- (4c) The development of a standard nomenclature for specific language names and for reconstructed entities and for taxonomic levels. (Merritt Ruhlen's *A Guide to the World's Languages* might be a good place to start.) Some linguists change a language's name every time they hear a new pronunciation of it or at the first suggestion that someone is offended by it. We need to become more like biology and adopt *some rules*. Family, stock, branch, cluster, phylum, super-phylum, mega-phylum, et cetera. We need agreements and a rationale.

5. Limit the publication of *Mother Tongue* to once a year. *Mother Tongue* should have the attributes of a journal and contain high quality articles, mostly those with \*MT Treatment. It should sum up a year's research in prehistoric linguistics and perhaps a few related matters. Actually, nowadays that is not a great deal. It might simply be called "Linguistic Prehistory". Thus, we must reward the efforts of contributors, by ensuring that this will be a well-respected, if not a prestigious, journal. One that helps a person get a promotion or tenure.

- (5a) Publish a small newsletter on a periodic basis for more routine matters, perhaps including references to important archeological and biogenetic research.

6. Change the By Laws to merge the offices of President and Treasurer, Vice President and Secretary, until such time as working officers can be elected to the Treasurer and Secretary posts.

7. Discourage membership viewed as a subscription. Discourage passive membership or mere readership. We do not intend to become what Americans call a "magazine". This will result in the probable loss of the great bulk of our members. Yet it is a sad conclusion that keeping most of them enrolled squanders our energy. We misdirect our efforts when we try to be *Science News*.

8. What to do with the core group of really interested but

passive members is a problem. This includes most of the non-linguists. We propose that one solution to the problem is to follow the logic of the situation and let each of them decide for him/herself. There will be the annual journal and occasional small newsletter which they can read — for a price. They might be Passive Members of ASLIP (see 10c below). They can always become Active Members by application — and becoming active.

9. The basic problem, *au fond*, is that there is a very limited number of genuine long rangers in linguistics — maybe 52 altogether — and 2 of the 52 are tied up with publishing. Besides that, another 4 of the 52 are tied up by illness or advanced age, while 5 of the 52 are handicapped by obsessions with binarisms that generate non-valid taxa. One is paralyzed by timidity, while another 2 work hard but lack focus, hence work to no avail. Perhaps 4 work only within Nostratic; that's similar to working within Niger-Congo. Then there is a bunch who say they are long rangers but actually do nothing. Even though one can count 8 Russians by the above criteria, only Diakonoff seems able to cooperate internationally. Emigré Muscovites of the recent diaspora have grown silent, no doubt due to social pressures to conform to the hyper-caution in their new departments. *All* are silent save a few who have started attacking Greenberg. Just to survive? There are other Russian cooperators, but they work only within Afro-Asiatic. That also could be considered as long range work (see 9a below).

(9a) Essentially, there are six genuine long rangers to do by themselves the inter-phyletic comparisons we need to advance. If we add Allan and Hal to that sextet, we increase their numbers by a third. Let us add a few who work broadly within very large or differentiated phyla, such as Kay Williamson, Roger Blench, Robert Blust, or Christopher Ehret. Compared to your average Indo-Europeanist or Americanist, they are genuine long rangers. Yes, even M. L. Bender, who savages Greenberg and Fleming at every opportunity, is a genuine long ranger, albeit currently confused about who he is.

(9b) One of us will have to start work on Indo-Pacific, which Greenberg started. It appears that no one is working on it or has worked on it for years. Roger Blench made some inquiries on the subject and pronounced Indo-Pacific dead in the water. The Aussies should have done that work, but their characteristic boldness has been stifled by a governing paradigm of hyper-caution. A real pity!

10. Membership and admission to membership and dues. These are the realizations of the principles sketched out above. A member should be active, not a passive reader, but not necessarily in linguistics. *Activity that helps seems most crucial*. We cannot anticipate all the marvelous things that members might propose to call activities, but we can at least outline some major ones, to wit:

- Do some specific long range comparisons on languages.
- Do journal research or journal surveying or conference reporting and write small summaries for distribution to colleagues. Relevance is the key to this. For example, review the literature on Dilmun (Eric de Grolier suggests this). Or the archeology of Tibet.
- Be on a committee that applies in ASLIP's name for grants, conference funding, travel, or the like. For example, trying for funds to send some biogeneticists to western Ethiopia, northern Chad Republic, northeast Nigeria, southwest Oman, etc.
- Help set up cooperation and coordination of data bases in long range historical linguistics. Several people could do a lot of valuable work, but difficult work, on this.
- Serve on a committee to create ASLIP standard symbols for phonetic transcription. Much consulting internationally and must know phonetics. We are not neglecting phonemics or generative phonology here — they are just not part of this problem.
- Serve as an officer, e.g., Secretary or Treasurer.

(10a) While admission to membership is automatic for a few people because of their official status within ASLIP, everyone else can be admitted to membership by a simple application, statement of activity, and payment of dues. The application forms should appear in *Mother Tongue* 24, roughly mid-January, 1995. Officers and Directors on the Board have been *elected* at the annual meeting in 1994 and so will remain members of ASLIP at least until April 15th, 1995. If a person lacks an application form, s/he can write a letter to one of the officers stating what activity s/he will be involved in. Still we must insist that admission to membership has to be applied for.

(10b) Dues are only to facilitate xeroxing, mailing, office supplies, etc. While there will only be one annual large production, there will be lots of copying and sharing and moving around of material. These are costs of our business. Our officers receive no money, and our Directors do not get paid to attend meetings. Normally, it costs money to be an active member. We guess that \$25 a year is a proper assessment. Let's have feedback on this.

(10c) There is a suggestion, not a proposal here, that another category of member be stipulated. The passive people might be interested in joining if they do not have to do any work. They would only get the annual plus the small occasional newsletter but that might be enough for them. So, estimating the cost of the work they might have done, we set a possible fee of \$50 for passives. There are a few scholars, now not so active, who have done a great deal for long

range comparison in the past. We will recommend that the Board of Directors make them Life Members, i.e., no dues.

- (10d) Persons from countries with currency problems will continue to be carried by their colleagues. However, they will *not* be carried as passive members. They must apply for active membership.

Okay? Give us your opinions, if you wish.